

VOLUME 100 • NUMBER 3 • JUNE 1995

The American Historical Review

AMERICAN HISTORICAL ASSOCIATION



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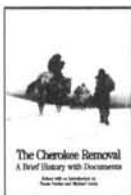
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The American Historical Review appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 (202-544-2422) and is printed and mailed by Cadmus Journal Services, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located at 914 Atwater, Indiana University, Bloomington, Indiana 47405 (812) 855-7609.

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Postmaster: Please send notification (Form 3579) regarding undelivered journals to: American Historical Association, 400 A Street, S.E., Washington, D.C. 20003. Publication identification number: *American Historical Review* (ISSN 0002-8762).

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FOR PERMISSION TO REPRINT:

Contact Sharon K. Tune, Executive Associate, American Historical Association, 400 A ST SE, Washington, DC 20003. Phone (202) 544-2422, fax (202) 544-8307.

Second-class postage paid at Washington, D.C., and at additional mailing offices

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Contributors

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Greg Dening is Emeritus Professor of History at the University of Melbourne. His long education in philosophy, theology, history, and anthropology has resulted in a distinguished career of teaching and writing. His

books, which include *Islands and Beaches* (1988) and *Mr. Bligh's Bad Language* (1993) have been anthro-historical in character. Forthcoming titles are *The Death of William Gooch* (University of Hawaii Press, 1995) and *Performances* (Melbourne University Press, 1996). A fellow of the Academy of Social Sciences of Australia and the Royal Historical Society of Victoria, Dening studied philosophy and languages at Loyola College, Watsonia, earned a master's in history from the University of Melbourne, a degree in theology from Canisius College, Pymble, New South Wales, and a master's and doctorate in anthropology from Harvard University. From 1969 to 1971, he served as Senior Lecturer in history and sociology at LaTrobe University and was Max Crawford Professor of History at the University of Melbourne from 1971 to 1991. His interests range across historical anthropology and anthropological history.

R. David Edmunds is of Cherokee descent and currently serves as a professor of history at Indiana University. He is the author or editor of seven books, including *The Potawatomi: Keepers of the Fire* (1978), *The Shawnee Prophet* (1983), *Tecumseh and the Quest for Indian Leadership* (1984), and *The Fox Wars: The Mesquakie Challenge to New France* (1993, co-authored by Joseph L. Peyser). During 1990–1991, Edmunds served as the acting director of the McNickle Center for the History of the American Indian, at the Newberry Library, Chicago. He is co-authoring a textbook in Native American history and in 1995–1996 will hold a Guggenheim Fellowship to conduct research on the role of Potawatomi entrepreneurs in the trans-Mississippi west.

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Critical Theory (1989), and *Representing the Holocaust: History, Theory, Trauma* (1994). In 1982, he co-edited with Steven L. Kaplan *Modern European Intellectual History: Reappraisals and New Perspectives* (Cornell University Press). He is currently working on problems bearing on the interaction between psychoanalysis and history.

Mark H. Leff is an associate professor of history and a fellow at the Program for the Study of Cultural Values and Ethics at the University of Illinois, Urbana-Champaign. He received his doctorate from the University of Chicago in 1978, studying with Barry Karl. Leff's interests center on U.S. public policy in the twentieth century. Author of *The Limits of Symbolic Reform: The New Deal and Taxation, 1933–1939* (1984) and "The Politics of Sacrifice on the American Home Front in World War II," *Journal of American History*, 77 (March 1991), he is now at work on a study of the power relationships and objectives animating American mobilization policy in World War II.

Nicholas Lemann, national correspondent for *The Atlantic Monthly*, was born and raised in New Orleans and began his journalistic career there as a reporter for *The Vieux Carré Courier*, an alternative weekly newspaper. He graduated from Harvard College in 1976 with a degree in American history and literature. He has worked at *The Washington Monthly*, *Texas Monthly*, *The Washington Post*, and, since 1983, *The Atlantic Monthly*. His work also appears often in other publications. He is the author of three books—most recently, *The Promised Land: The Great Black Migration and How It Changed America* (1991), a history of African-American migration to the big cities and of the War on Poverty and other government social initiatives. Lemann is now at work on a book about meritocracy in the United States.

Earl Lewis is professor of history and Afroamerican and African Studies at the University of Michigan. The author of *In Their Own Interests: Race, Class, and Power in Twenti-*

eth Century Norfolk (1991), Lewis is general editor with Robin D. G. Kelley of an eleven-volume history of African Americans for young adults, forthcoming from Oxford University Press. He studied at the University of Minnesota with Russell R. Menard, John Modell, and Clarke Chambers. His several essays on race, history, and identity include "Connecting Memory, Self, and the Power of Place in African American Urban History," *Journal of Urban History*, 21 (March 1995). Lewis is currently finishing a book on history, identity, and the multiple meanings of race in American life.

Patricia Nelson Limerick is a professor of history at the University of Colorado, Boulder, and recent recipient of a MacArthur fellowship. She is the author of *Desert Passages* (1985), *The Legacy of Conquest: The Unbroken Past of the American West* (1987), with Richard White *The Frontier in American Culture* (1995), and, with Edward Ayers, Stephen Nissenbaum, and Peter Onuf, *All over the Map* (forthcoming). Her field is Western American history, with a particular interest in the issues involving both ethnic history and environmental history. This essay represents her attempt to step away from the voluminous literature about Frederick Jackson Turner and face his work directly.

Dorothy Ross teaches American intellectual history at Johns Hopkins University, where she is Arthur O. Lovejoy Professor of History. She is the author of *G. Stanley Hall: The Psychologist as Prophet* (1972), *The Origins of American Social Science* (1991), and the editor of *Modernist Impulses in the Human Sciences, 1870–1930*, a collection of essays by David A. Hollinger, Richard Rorty, Theodore Porter, Stefan Collini, Jan Goldstein, Philip Pauly, and others that bring modernist/postmodernist interpretations to bear on the human sciences. Her work has concerned historical consciousness in the United States as well as the history of social thought and the human sciences; she is currently working on the

gendered models of social knowledge produced by Jane Addams, Florence Kelley, and the colleagues at Hull House.

Gordon S. Wood is University Professor and professor of history at Brown University. He studied early American history at Harvard with Bernard Bailyn and teaches courses in the histories of the colonial period, the American Revolution, and the early Republic.

Wood is the author of *The Creation of the American Republic, 1776–1787* (1969), which won a Bancroft Prize and the American Historical Association's John H. Dunning Prize; and *The Radicalism of the American Revolution* (1992), which won the Pulitzer Prize in History and the Ralph Waldo Emerson Award of Phi Beta Kappa. He is now working on a volume on the early Republic for the Oxford History of the United States.

Editors of the *American Historical Review*

J. Franklin Jameson	1895–1901
Andrew C. McLaughlin	1901–1906
J. Franklin Jameson	1906–1928
Dana C. Munro	1928–1929
Henry E. Bourne	1929–1935
Robert Livingston Schuyler	1936–1941
Guy Stanton Ford	1941–1953
Boyd C. Shafer	1953–1963
W. Stull Holt	1964
Henry R. Winkler	1965–1968
R. K. Webb	1968–1975
John Duffy, interim editor	October 1975–April 1976
Robert F. Byrnes, interim editor	June 1976
Robert E. Quirk, interim editor	October 1976–February 1977
Otto Pflanze	1977–1985
David L. Ransel	1985–1995

Associate Editors at Indiana University

Barbara Hanawalt	October 1976–October 1977
Paul Lucas	April 1978–June 1980
James Diehl	October 1980–December 1981
Helen Nader	February 1982–June 1986
Ann G. Carmichael	October 1986–October 1988
Ellen Dwyer	December 1988–December 1991
Leah Shopkow	February 1992–December 1993
Peter F. Guardino	October 1994–Present

DAVID L. RANSEL

COMMEMORATIONS OF THE *AHR* have dwindled in size as the journal has matured. The first and longest appeared in 1920 on the occasion of the twenty-fifth anniversary. J. Franklin Jameson, the first editor, composed an elegant seventeen-page essay recounting the origins and achievements of the *AHR*.¹ At the fiftieth anniversary in 1945, editor Guy Stanton Ford had hoped to devote part of the volume to the semicentennial, if only to reprint and extend Jameson's 1920 essay. But, Ford remarked cryptically, "That possibility became quite uncertain." He decided instead to reprise in two pages of his annual report the circumstances of the journal's creation as presented earlier by Jameson. In doing so, he reported erroneously that "twenty-six men attended" the founding meeting of April 1895, forgetting to mention that one of the "men" was Lucy Salmon of Vassar College.² This omission provided a portentous beginning to the postwar era, a period that saw a significant decline in opportunities for women in the historical profession.

The seventy-fifth anniversary passed without so much as a mention either in the *AHR* itself or, more surprisingly, in the annual report of the editor, R. K. Webb. The year was 1970, and the Association was in upheaval over the issue of adopting an official position on the Vietnam War, not to mention implementation of rules to prevent direct democracy at the annual Business Meeting. The leaders of the AHA apparently had so many other important matters on their minds that they forgot to tip their hat when passing this *AHR* milestone.

For the one hundredth anniversary, we have planned two special issues. This first one is devoted primarily to essays on U.S. history, the second (to follow in October) will feature contributions covering for the most part topics outside the field of U.S. history.

We asked the contributors to both numbers to think about the changes that have occurred in the understanding and writing of history in their fields over the past century.³ For most of them, the task was twofold: first, to read through the

¹ J. Franklin Jameson, "The American Historical Review, 1895-1920," *American Historical Review*, 26 (October 1920): 1-17.

² Guy Stanton Ford, *AHR*, 50 (April 1945): 645-47.

³ The idea of the usual jubilee-type publication in which the editors and others survey the history and achievements of the journal had little appeal. Readers interested in these matters can turn to other sources, including the essay by Jameson mentioned earlier, articles by Morey D. Rothberg, "To Set a Standard of Workmanship and Compel Men to Conform to It": John Franklin Jameson as Editor of the *American Historical Review*, *AHR*, 89 (October 1984): 957-75, and Jacqueline Goggin, "Challenging Sexual Discrimination in the Historical Profession: Women Historians and the American Historical Association, 1890-1940," *AHR*, 97 (June 1992): 769-802, a book by Margaret F. Stieg, *The Origin and Development of Scholarly Historical Periodicals* (University, Ala., 1986), and Peter Novick's

early volumes of the *AHR* and identify what our predecessors thought of as a historical question, explaining how they defined issues and what methods they used in treating them; and, second, to jump ahead one hundred years and tell us what historians in the contributors' fields now understand as proper objects of study and what methods and approaches they are currently using.⁴ In other words, the contributors were asked to re-create two intellectual settings, the turn of the last century and the turn of ours, without mapping the terrain in between. What we hoped for were current state-of-the-field essays that also took the measure of how far a particular field had or had not moved away from the definitions and concerns of the first generation of professional historians. The contributors interpreted this task with flexibility and, in following their own intellectual bents, have produced a readable and varied collection of essays within the broad framework proposed.

Readers who spend time with the early numbers of the *AHR* will find much of interest, including a review by Frederick Jackson Turner of Theodore Roosevelt's *The Winning of the West* followed by Theodore Roosevelt's review of Mary P. Follett's book *The Speaker of the House of Representatives*.⁵ They will find articles that express a faith in the scientific accuracy of historical reconstruction and a surprisingly contemporary essay by Henry Adams about the impossibility of achieving such accuracy. Readers will search in vain, however, for references to many topics of current interest to historians. The absences are palpable. Not only is there little about African Americans (even if slavery as an institution is treated), women, Hispanics, or just ordinary people generally, but whole eras of history are also passed over in silence. The primary concerns of our first generation of professional historians scarcely ranged beyond the Middle Ages, early America to the revolutionary era, including the French Revolution, and recent U.S. history. They seemed to see their primary task as tracing the lineages of American democracy. As Thomas Bender remarked when the Board of Editors was discussing these issues, the founding generation had "a genetic understanding of modern society." Carole Shammas added that, yes, the essays were all "liberation tales."

It would have been impossible to build our centennial issues on analyses of history then and history now if we had limited our contributors to only those topics represented in the early volumes. The mix of essays therefore includes long-established topic areas, such as political history and early America, and more recently developed emphases, such as Native American and African-American history.⁶

Two of the ten authors for this June number were given more specific tasks than the others. Dorothy Ross was asked to prepare a reply to the inaugural essay of the *AHR* by William M. Sloane, "History and Democracy." (See below for more about

splendid book about the profession more generally, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, 1988).

⁴ I want to thank my colleague Jeffrey Wasserstrom for this specific idea.

⁵ *AHR*, 2 (October 1896): 171–78.

⁶ An essay on Chicano history and the U.S.–Mexican borderlands, which arrived too late for this issue, is planned for the second centennial issue in October.

each contribution.) Sloane began the very first issue with this powerful and wide-ranging article, which raised many of the same questions that concern historians today, even if Sloane's answers were shaped by the scientific thought of his era. The essay is so rich in observations attuned to its time that, in addition to being the focus of Ross's article, it is referenced in many other contributions.

A special assignment was also given to Greg Denning, the author of the final contribution to this June number. Denning, an Australian, was trained as an anthropologist as well as a historian and is very familiar with the United States. As an outsider with impressive qualifications for observing the exotic, he was asked to take a look at what our tribe has been up to for the past hundred years.

NOW TO A SUMMARY of the contributions for this first issue.

Dorothy Ross uses William Sloane's essay as a vantage point from which to examine grand narrative in American historical writing. She first looks back to the construction of the American grand narrative in the work of George Bancroft, a story told in the literary mode designated by Northrup Frye as "romance." (Sloane did not disagree with this story of Western progress and American exceptionalism but saw the need for it to be fleshed out and supported by a new scientific historiography.) Ross then discusses the decline of the romantic grand narrative in the work of the Beards, who wrote it as a comedy, and in the work of the postwar and Cold War historians, who adopted an ironic mode. Despite efforts by post-1960s historians of the dispossessed and by liberal historians of synthesis to reconstitute a romantic grand narrative, it was no longer persuasive. The pivotal point in the evolution of rhetorical strategies, Ross believes, can be found in the ironic historians of the postwar era. They punctured the American-centered romance once and for all. The postmodernist turn of our own day offers more narrative strategies but also more uncertainty.

Gordon S. Wood takes as his foil an essay by Henry Adams in the first issue of the *AHR*. Adams's pessimism about the accuracy of historical writing was not in keeping with his generation's optimism about composing a thorough and precise American history. Wood credits late nineteenth-century historians with setting forth most of the conventions and rules by which history writing is practiced today. Even so, he notes a great difference between that era and our own. Histories of early America in the second half of the last century were rudimentary, he writes, often the work of amateurs, and focused on the origins of the nation. The first generation of professionals devoted much of their time to collecting documents and removing the errors introduced by their predecessors. By contrast, Wood continues, present-day histories are more sophisticated and more varied in their subject matter. Early American history today not only speaks about people formerly excluded but is also framed as part of a larger world of the Atlantic basin, including Europe, Africa, and Hispanic America. Despite some expressions of despair that echo the pessimism of Henry Adams, most contemporary historians of early America continue, Wood concludes, to have faith in and hold to the

standards of objectivity, accuracy, and documentation that the founding generation of professional historians established.

Frederick Jackson Turner was president of the AHA and a frequent contributor to the *AHR*. According to **Patricia Nelson Limerick**, Turner's famous 1893 Frontier Thesis has proved resistant to seemingly devastating attacks on its evidence and assumptions, including her own. Recent efforts to demonstrate the weaknesses of the Frontier Thesis have only strengthened its reputation. Limerick shows that Turner was his own most telling critic, but he scattered arguments for a "Frontier Antithesis" in a variety of articles. He never directly confronted, much less rejected, the 1893 thesis that had won him fame and professional success. This choice cut against Turner's avowed commitment to the use of history as a means of explaining present reality. (The study of history for its own sake he scorned as "antiquarianism.") Even though the Frontier Thesis made less and less sense as an explanatory model for the industrializing America of Turner's day, he was unwilling to rethink it and find a model of past experience more appropriate to ordering present experience. Limerick contends that the problem in this type of historical analysis is not the past but the present. She encountered in her New West thesis a dilemma similar to Turner's. Like him, she learned that it is not the past that is messy and in need of a known present to give it order; rather, the present is the unknowable and uncertain dimension, and therefore offers no clear guide to sorting out the past. Presentism, she adds, also flattens the drama of history by making events in the past appear inevitable.

In the Frontier Thesis, if not in its Antithesis, American Indians received scant attention. Similarly, the early volumes of the *AHR* contained few references to Indians. **R. David Edmunds** attributes this silence to the belief at the time that Native Americans were on the brink of cultural and physical extinction. Edmunds explains that until the decay of the postwar consensus school in the 1960s, the history of American Indians remained in academic limbo and Indians were considered only as impediments to or objects of other actors. The events of the 1960s and beyond—the civil rights movement, the Vietnam War, the emergence of ethnohistory—were accompanied by an explosion of interest in Native American history. Edmunds surveys the output of this new era, finding its principal concerns to be with pre-Columbian history, demography, multi-ethnic societies, Native American diplomacy, Indian leadership, Native American identity, Indian education, women, and the problems connected with developing an accurate and articulate "Indian voice."

Because the early volumes of the *AHR* featured work by non-academic historians (Henry Charles Lea, Theodore Roosevelt, William R. Thayer, and others), we thought it important to include contributions by leading non-academic historians of today. The first of two such contributions is an interview with the documentary filmmaker **Ken Burns**. Although documentary film is almost as old as the *AHR* itself, historical film did not have a place in the pages of the journal until very recently. The first sustained discussion of history on film did not appear here until

1988. This discussion was followed the next year by the debut of an annual Film Review section. In subsequent years, special *AHR Forums* were also devoted to important feature films such as *JFK* and *Malcolm X*. We asked a historian of film, **Thomas Cripps**, to conduct an interview that would explore Burns's understanding of his position in the history of documentary film. In addition, Burns has much of interest to say about the work of academic historians, the relationship of visual media to politics, and our country's need for a usable past that is both accessible to a large number of people (unlike most academic history) and presented in a way that will allow us to shape a common identity.

Earl Lewis looks at African-American history. Although W. E. B. Du Bois made major contributions to early volumes of the *AHR*, Lewis rightly points out that the history of black Americans, apart from treatments of slavery as a legal condition, was allowed little scope in the journal's first half-century. The editors did not even find space for a review of Benjamin Brawley's pioneering study, *A Social History of the American Negro* (1921). Lewis compares this record with the even more limited coverage given to African-American issues in political science journals and gives the *AHR* credit for playing an important role in discussions about the history of slavery. Much of the exciting work on this topic and on post-emancipation African Americans, however, took place elsewhere, and only recently has the *AHR* moved back into the picture. Lewis finishes with a discussion of recent work in his field and argues the need for a clear distinction between race formation and identity formation. In this regard, he notes the importance of seeing historical actors as multipositional. African-American history is one of "overlapping diasporas" in which identity formation is highly complex.

Nicholas Lemann, national correspondent for *The Atlantic Monthly* magazine, is the author of our second contribution by a non-academic historian. Lemann believes that the enshrinement of the Ph.D. degree as the necessary credential for academic historians, the de-emphasis of narrative in their work, and ever more specialized investigations have driven away a popular readership. Non-academics now write popular history for the large market of history readers, and a number of writers are able to make a living at this work. Like Lemann himself, they choose subjects of important current social and political concern and build their books and articles on narrative description. Often, their work is based more on interviews than on documents. Lemann illustrates these general observations by reference to his own work. He relates the process through which he selects topics for study, researches and writes them. He also assesses the shortcomings of non-academic history, which he identifies as the power of narrative to mislead and the difficulty of combining analysis with narrative.

Dominick LaCapra's assignment was to compare the reading of texts then and now. He begins with a brief discussion of the two very different articles by William Sloane and Henry Adams in the inaugural number of the *AHR*. These essays are used to illustrate interpretive problems in the relationship between past and present and their association with approaches to reading, or the much-discussed

“linguistic turn.” LaCapra then analyzes five types of reading, noting that while historians may mingle two or more of the types in their current practices, the initial choices determine the character of their inquiries. The types are denial or repression of reading (when historians fail to recognize reading as a problem and do not analyze texts), synoptic reading (a more critical relationship to texts but one still using them primarily as a repository of unproblematic information), deconstruction (when focus is on the response of the reader to the text—here, LaCapra includes a detailed discussion of the difference between the approaches of Paul de Man and Jacques Derrida), redemptive reading (when historians acknowledge the issues that engage deconstructionists but nevertheless seek to redeem the meaning of past experience—at this point, LaCapra turns to a lengthy discussion of the pathbreaking *AHR* article by John Toews in 1987). Finally, the fifth type, which LaCapra calls dialogic reading, is the one he has developed in his own work. He believes that it integrates desirable aspects of other types and contends, more generally, that a mutual rethinking of psychoanalysis (especially the concept of transference) and historiography can offer insight into the interplay between past and present.

Political history, the assignment given to **Mark H. Leff**, might at first seem the easiest to address because of the prominence of political history in the first volumes of the *AHR*, indeed, the assumption in those days that history was almost exclusively politics of the past. While some contributors have had to overcome the silence of the early *AHR* on their topic, Leff has to shout over the clamor of the current cultural wars and the battles about who will succeed in defining our political history. He outlines two options available to political historians who wish to reclaim their central place in the discipline. They can follow the path of the right-wing ideologues who are attempting to freeze in place a nostalgically drawn, triumphalist postwar consensus history. Here, Leff considers the recent battles over the construction of the Enola Gay exhibit at the National Air and Space Museum and the debate over the National Standards for United States History. Alternatively, political historians can engage in a process that many of them have already started, namely, the path of cross-fertilization of political history with political science, gender analysis, and social history. According to Leff, the effort to reach out beyond traditional practices is positioning political historians to play a constructive and reintegrative role that would allow them to overcome the profession’s fragmentation and further the contribution of historical study to democratic civic life. Leff finishes by surveying works that take this new and constructive approach.

Greg Dening rounds out the issue with his observations on the doings of our tribe as reflected in the pages of the *AHR* during the past century. He finds that we have not changed our rites and totems to any large degree, but, within a fixed territory and set of practices, we have shown ourselves capable of both silliness and grandeur.

Grand Narrative in American Historical Writing: From Romance to Uncertainty

DOROTHY ROSS

WILLIAM M. SLOANE, soon to be Seth Low Professor of History at Columbia University, began the first number of the *American Historical Review* by noting the existence of “widespread discontent with the results of historical study as pursued to-day.” If that situation seems familiar, Sloane’s response to it could not be more different from our own. Sloane confidently asserted that the historians’ new professional project would put those discontents to rest. A chief source of his confidence was something no longer available to us a century later: an “evolutionary philosophy” that disclosed the unity and continuity of all history, made possible a true science of history, and established “the real landmarks of all time, and exhibit[ed] them in their proper proportions as to the ascent of man.”¹ The most striking difference between Sloane and ourselves is his assurance that he knows the grand narrative² of all history and that it is a narrative of progress.

The absence of an assured grand narrative in current American historiography is hardly news. I would like to suggest, however, that its absence is still an active presence. As a model to be approached or avoided, Sloane’s American-centered story of Western progress still animates and troubles contemporary historiography. The difficulties created by this presence lie, I believe, at the intersection of political hopes and narrative possibilities. Because the politics and poetics of uncertainty are necessarily shaped by what has been certain, I will begin by looking briefly at the formation of grand narrative in the nineteenth century and at the way Sloane and the leading Progressive historian Charles Beard reformulated it, before turning to the 1950s, the point at which our own problems began. That moment of American triumphalism initiated a political mentality and a retreat from grand narrative that have colored historical practice ever since.

I would like to thank Kenneth Cmiel, Toby Ditz, David Hollinger, Linda Kerber, Douglas Rossinow, Gabrielle Spiegel, Judith Walkowitz, and Ronald Walters for their instructive comments on this essay, as well as the students in my graduate seminar who went through much of this material with me: Lawrence Charap, Ivan Grabovac, Hunter Heyck, and Lyric Wallwork-Winik.

¹ William M. Sloane, “History and Democracy,” *American Historical Review* (hereafter, *AHR*), 1 (October 1895): 1–23, esp. 1–2.

² I use the term “grand narrative” here to mean the story (with beginning, middle, and end) of all humanity. This is an adaptation of the term as used by Allan Megill (adapted in turn from Jean-François Lyotard) in “‘Grand Narrative’ and the Discipline of History,” *A New Philosophy of History*, Frank Ankersmit and Hans Kellner, eds. (Chicago, 1995). Megill differentiates “grand narrative” from “master narrative,” the story of one segment or nation. In practice, however, the master narratives of the United States and Europe that I will be discussing are integrally related to grand narratives of the progress of humanity and generally imply their existence.

My intention is not to provide a history of how we got to our current situation but to mark some of the key changes in the character of grand narrative and its role in American historical writing. American historians of other countries have been influenced by this grand narrative and share some of its dilemmas, but their involvement in other historical cultures makes their outlook somewhat different, so I will concentrate chiefly on historians of the United States.

THE GRAND NARRATIVE that structured American historiography in the nineteenth century was composed of two closely intertwined strands. One was the story of Western progress, a liberal story of growing commercial development, representative political institutions based on democratic consent, and the advance and diffusion of knowledge—processes that were projected to remake the entire world. The second was the liberal/republican story of American exceptionalism, which seated world progress in the American nation. The special place of the United States in this story was attributed in part to favorable historical conditions that allowed it to form a New World antithetical to the old: the heritage of Anglo-Saxon institutions, the republican frame of government, the continent of uncultivated land, the opportunity offered by a free market of small producers. But specialness derived fundamentally from divine favor, a favor that began with the Puritan mission to New England and was sealed in the American Revolution and Constitution. The country's unique foundation located it in millennial as well as historical time, freeing it from the ills of Europe and guaranteeing it an ideal future, exemplary for the world.³

This American-centered grand narrative thus lies in the literary territory Northrop Frye has designated *romance*.⁴ As Frye understands it, romance is not only a type of narrative plot but also a broader mode or tendency that shapes the literature lying between myth on the one side and naturalism on the other and partaking of both. It is the tendency “to displace myth in a human direction and yet, in contrast to ‘realism,’ to conventionalize content in an idealized direction.”⁵ Whereas realism takes place on a horizontal plane, romance is plotted as an ascent from a lower to a higher world; the lower one demonic or regressive, the higher one ideal or progressive. Frye suggests that these narrative principles of ascent from a lower to a higher world and their polarization into demonic and ideal operate in the plots of both romance and comedy. The romance plot is written from the standpoint of the human actor engaged in a mythic quest for identity. The hero embarks on a quest to achieve or recover an identity that is prefigured at the beginning and at the end enters an Edenic world, a world of mutual love

³ Dorothy Ross, *The Origins of American Social Science* (Cambridge, 1991), pt. 1.

⁴ While I make use here of Frye's insight into the principles of narrative modes and genres, I do not want to import along with it his romanticist, structuralist, and scientistic assumptions. I assume that in historical writing as in other kinds of literature these principles emerge from human action in history. One consequence is that historical texts often defy neat categorization, as Frye's substantive analyses, if not his theoretical leanings, make clear. For an excellent critique of Frye's ahistorical assumptions, see Frank Lentricchia, *After the New Criticism* (Chicago, 1980), chap. 1.

⁵ Northrop Frye, *Anatomy of Criticism* (Princeton, N.J., 1957), 136–37.

"within an order of nature which has been reconciled to humanity."⁶ Comedy is a social drama rather than a quest for identity, whether the divine comedy of the Bible that ends in the Last Judgment and the Kingdom of God, or a human comedy that moves from one kind of society to another promising greater social integration and freedom.⁷ Hayden White suggests that comedy can be understood as a qualification of the romantic vision of the world "in the interest of taking seriously the forces which *oppose* the effort of human redemption naively held up as a possibility for mankind in Romance." Still, comedy ends in reconciliation, the "seemingly inalterably opposed elements in the world . . . are revealed to be, in the long run, harmonizable with one another."⁸ In both, the romantic tendency operates to displace into the human world the polarization of good and evil and the happy ending in a redeemed world that belong to myth.

It is not surprising that romance should shape the grand narrative of history. With its conception of all human history as a single story, uniting past and future in coherent meaning, grand narrative is necessarily a mythic construction. It is also necessarily a political construction, a projection of historical hopes or fears that authorizes a contestable version of the world.⁹ For nineteenth-century Americans writing their national history, as for other writers of national stories, romance has, as Frye notes, been a favorite mode.¹⁰ While there were some Americans who denied the idea of special providential favor on secular, and others on religious, grounds,¹¹ many others saw their country as an actor both in secular history and in the Christian myth of divine redemption. When the millennium came to be understood in increasingly secular terms as human progress, the tools of modern historiography could be used to illuminate the patterns of ascent to divine/human fulfillment.

George Bancroft gave the American-centered grand narrative its most popular and compelling form. He cast it as a romance, the story of the achievement of America's divinely ordained identity. Beginning his history of the United States with the Puritan mission to New England, he ended with the revolutionary victory in 1782 and the achievement of independence from Europe, national unity, and democratic freedom.¹² Indeed, Sloane's summary of Bancroft's history brings out clearly its romantic elements:

⁶ Northrop Frye, *The Secular Scripture: A Study of the Structure of Romance* (Cambridge, Mass., 1976), 49, 54, 80, 139, 149. As Frye puts it, "Identification and self-recognition begin when we realize that . . . the great twins of divine creation and human recreation have merged into one, and we can see that the same shape is upon both" (p. 157).

⁷ Frye, *Anatomy*, 163, 169.

⁸ Hayden White, *Metahistory: The Historical Imagination in Nineteenth-Century Europe* (Baltimore, Md., 1973), 9–10. For White's more recent discussion of historical narrative, see *The Content of the Form: Narrative Discourse and Historical Representation* (Baltimore, 1987).

⁹ For history as an authorizing narrative, see White, *Content of the Form*, 19–25.

¹⁰ Frye, *Secular Scripture*, 8.

¹¹ For an example of a secular dissenter, see Donald Fleming, *John William Draper and the Religion of Science* (1950; rpt. edn., New York, 1972). On religious critics, see Mark Y. Hanley, *Beyond a Christian Commonwealth: The Protestant Quarrel with the American Republic, 1830–1860* (Chapel Hill, N.C., 1994).

¹² George Bancroft, *History of the United States from the Discovery of the American Continent*, 10 vols. (Boston, 1837–74); in 1885, Bancroft added a final volume on the Constitution to a revised edition of *History*. See also Dorothy Ross, "Historical Consciousness in Nineteenth-Century America," *AHR*, 89 (October 1984): 909–28; Sacvan Bercovitch, *The Rites of Assent: Transformations in the Symbolic Construction of America* (New York, 1993), chap. 6.

His book may be considered as a treatise on the evolution of liberty along the central axis: this axis is the land designated by Providence as fitted not for freedom's relative but for its absolute development. Its heterogeneous population brought and brings from all other lands the elements of national character, and by this compulsion of origins the environment, though eliminating all that cannot be assimilated, retains all useful elements, incorporating them into an intricate but orderly whole.

Here is the formation of the United States as a human quest on divinely ordained ground, and here, as Sloane put it, ran the democratic line of historical development by which "the possibility of human perfectibility may be realized."¹³ Retrogressive and ideal worlds are polarized as the United States separates itself from the Old World and the American environment selects only "useful elements." In romance, this process often takes the form of exile. The search for identity begins in the exile from home, as it began for the New England Puritans, and ends when the evils encountered in the world are cast out. Thus Bancroft's contemporaries Francis Parkman, J. L. Motley, and William H. Prescott had to turn to Europe and the far reaches of the North American continent to compose tragic histories of failure, their failures proving the rule of American and, ultimately, world progress.¹⁴

The American grand narrative and its trust in special providential favor survived intact until the end of the century, when the weakening of religious belief and the industrial transformation of society called it into question. Many of the historians who established the profession wished to separate history from its divine background and to recover the past realistically, free of romance and its literary forms. History was to become science, not literature; Bancroft was kept at some distance. As John Higham showed, however, these new professionals were also eager to keep their ties with the larger public, which supported the historical enterprise and regularly gave them ceremonial positions.¹⁵ They must have had such a mediating purpose in choosing to open the *Review* with William Sloane, a professor and writer of monographs whose main work was a popular history of Napoleon and who "resembled the diplomat and man of affairs rather than the typical professor."¹⁶

More to our point, Sloane had served as private secretary and research assistant to Bancroft, and, unlike many of his colleagues, Sloane believed that science should support the edifice built up by literary history, not overturn it. Modeled on embryology rather than Darwinian theory, his evolutionary philosophy portrayed humanity as a single organic unit engaged in a progressive movement from barbarism to ever higher civilization. While scientific laws governed the movement from stage to stage, historical conditions and human actions differentially encouraged or halted progress.¹⁷ Sloane believed that this evolutionary frame

¹³ William M. Sloane, "The Science of History in the Nineteenth Century," *Congress of Arts and Science: Universal Exposition, St. Louis, 1904*, Howard J. Rogers, ed., 8 vols. (Boston, 1905-07), 2: 38-39.

¹⁴ David Levin's *History as Romantic Art: Bancroft, Prescott, Motley, and Parkman* (Stanford, Calif., 1959) remains the best study of the major historians of the nineteenth century.

¹⁵ John Higham, *History: Professional Scholarship in America*, rev. edn. (Baltimore, Md., 1989).

¹⁶ For biographical information, see William R. Shepherd, "William Milligan Sloane," *Dictionary of American Biography*, 9: 214.

¹⁷ Sloane, for example, was anxious to defend American democracy against the European charge

solidified the story of progress centered on the state that had been developed during the nineteenth century. He believed also that Bancroft had supplied its "capstone" by showing "God working in history" through American democracy. While the purely scientific historians often shared Sloane's belief in Western and American progress, they believed that the grand design was not yet known and required the patient amassing of facts to construct. For Sloane, "There is boundless room for advance in supplement, completion, illustration, but the plan has been sketched and the basis laid."¹⁸

Sloane was more in agreement with his generation on the political bearings of his historical program. In an era of labor conflict and Populist protest, he emphasized that American democracy was "essentially conservative and melioristic."¹⁹ Historical writing should support the conservative continuity of the American state against the encroaching evils of popular radicalism and modernity.²⁰ Sloane's adaptation of Bancroft's Jacksonian democracy to a conservative, Gilded Age purpose follows from the way Bancroft himself tied the voice of the people to gradual progress within the American frame.²¹ It also reveals the political ambiguity of romance's middle ground. As Frye suggests, romance carries a revolutionary potential in its presentation of an ideal world; but, when presented "under a guise of realism" that invites readers to identify with the society being portrayed, romance results in conservatism.²²

Nowhere is Sloane's continued attachment to romance clearer than in his identification of the problems Americans faced as incursions from abroad: "the dreadful system of African slavery which came with us from the Europe of a mercenary and mercantile age"; "the sludge thrown upon our shores from the governments of Europe, in the shape of the shiftless, stupid, and too often, criminal elements." American history was a process of absorbing what was useful into the nation's tissues and expelling the rest. "We have ploughed under and assimilated successive deposits of foreign immigrations, and have rendered them as beneficent as those made on Egypt's soil by the inundations of the Nile, feeling ourselves the stronger for their fertility and strength." Modernism and socialism, however, had to be expelled: "If such an attitude ever comes to America, we shall have anarchy and ruin."²³

that America was "a part of England switched off . . . what biologists call arrested development." Sloane, "History and Democracy," 2-4, 12.

¹⁸ Sloane, "Science of History in the Nineteenth Century," 39. Because of Sloane's cosmopolitan vision, he deprecated exclusive attention to the United States, and as a historian of both France and the United States, he thought the Teutonic theory of American democracy had been overemphasized: "No country is more than one wheel in the series which moves the hands on the dial-plate of human progress." Sloane, "History and Democracy," 3.

¹⁹ Sloane, "History and Democracy," 9.

²⁰ Ross, *Origins of American Social Science*, 53-77. Sloane likened the United States to "the explorer of the wilderness, who, while he presses forward, is ever turning to observe the landmarks behind him in order to direct his course by fixing a line from which he must not deviate." As the "must" suggests, this was as much political injunction as historical description. Sloane, "History and Democracy," 15.

²¹ Bercovitch, *Rites of Assent*, chap. 6.

²² Frye, *Secular Scripture*, 165-66.

²³ Sloane, "History and Democracy," 11, 15-16. "The old world today is weary of the past . . . yearning for modernity and futurity," and thus reaching for "socialistic legislation" and new experiments.

Sloane detailed his politics in the new *Review*, but he did not at that time fully specify the terms of the grand narrative he believed known for all time nor did he link Bancroft to it. He did so a decade later at the St. Louis Exposition when he mounted a full defense of his position. By then, he clearly felt under siege from the attackers of imagination and the writers of monographs. He closed his exposition of the American grand narrative with a challenge. "Either this achievement is all, or it is nothing; and our descendants must raze everything in order to begin anew the weary search for truth among the ruins of the past."²⁴ Sloane's historical successors chose to begin the "weary search," but they did not raze everything.

IF ROMANCE OCCUPIES THE TERRITORY between myth and naturalism, Charles and Mary Beard's masterwork, *The Rise of American Civilization* (1927), moved closer to the side of naturalism. Their grand narrative, too, was woven from the intertwined story of Western progress and the specially favorable conditions of U.S. history. But divinity was now gone from the scene, leaving only the contingent forces of history. From such materials alone, they could not claim certain knowledge of "the real landmarks of all time," as Sloane had, although they found good reasons to believe that history was progressing.²⁵ To the modern historical "consciousness of an irresistible flow of seconds, hours, days, years, and centuries," Charles Beard "coupled that fruitful eighteenth-century concept, the idea of indefinite progress." Yet Beard knew that the idea of progress had to be "coupled" to the open-ended contingencies of historical time, particularly after the carnage of World War I. Occasionally, that anxiety seeped into his prose: time was an "onrushing stream that devours us all," an infinity of "doomful possibilities."²⁶ In *Rise*, progress operates as a regulative principle of the narrative, but it is also a question that must be explicitly asked and answered.

The Beards plotted their history as comedy. It is a story not of America's achievement of a prefigured identity but of American society's struggle for progress. As social democratic liberals, they did not believe that the country's ideal identity had been established in the revolution and Constitution but that it had yet to be achieved by the transformation of the liberal capitalist order into a fully cooperative social democracy. Writing in the midst of the ever-faltering struggle to establish a modern welfare state, their story had to be one of ongoing effort that took seriously the oppositions to an idealized future.²⁷

The Beards had not altogether abandoned myth for naturalism, however. Under the aegis of naturalism, they believed history to be moved by deterministic chains of mechanistic cause and effect that seemed to belie the effort of moral

²⁴ Sloane, "Science of History in the Nineteenth Century," 39.

²⁵ Charles Beard, "Introduction," to J. B. Bury, *The Idea of Progress: An Inquiry into Its Origin and Growth* (New York, 1932); Nancy Cott, ed., *A Woman Making History: Mary Ritter Beard through Her Letters* (New Haven, Conn., 1991), 17, 37.

²⁶ Charles A. Beard, "Time, Technology, and the Creative Spirit in Political Science," *American Political Science Review*, 21 (February 1927): 1–11.

²⁷ On the Beards' politics and its context, see Ellen Nore, *Charles A. Beard: An Intellectual Biography* (Carbondale, Ill., 1983); Cott, *Woman Making History*; Ross, *Origins of American Social Science*, 339–46.

will. Such causes and effects seemed indifferent to morality: good issuing from evil sources and evil from good, human intentions producing unintended consequences. Under the aegis of romance, however, they wanted desperately to separate the good and the bad, to have moral effort rewarded and to have good motives yield good results.²⁸ On one level, they were able to do this by setting off “the people” against “the interests.” But, on a deeper level, the moral gap between cause and consequence remained, particularly because their economic interpretation of history required that worldly causes produce ideal results.²⁹ The Beards marked the moral gap between cause and consequence with rhetoric in the ironic mode. They used irony whenever good causes produced bad effects or bad causes good effects, when constructive capitalist energy produced destructive capitalist greed or when material self-interest produced idealistic talk.

Their use of irony in *Rise* marks the gap between a realistic history of the United States in the modern world, with all its moral ambiguity, and a wished-for history grounded in romance. If they could not close the gap, they did not abandon romance. Instead, they set out to write American history as “a history of a civilization,” a total history in the progressive mode that implicitly linked American progress to the progress of civilization.³⁰ Driven by this high purpose and their sense of its distance from the imperfect reality, their language reaches for mythic seriousness and romantic plenitude. As a result, irony shares the text with romantic rhetoric as the Beards alternate between—seem deliberately to stretch the reader between—the ironic all-too-human present and the mythic destiny of America.³¹ They may also have thought the romantic inflation suitable

²⁸ As Gordon Wood has shown, the mechanistic linking of good motives and good consequences was a product of the eighteenth-century effort to translate the Christian moral universe into the terms of naturalism. That Beard should still feel the lack of this moral link in the early twentieth-century world of Darwinian science suggests how firmly his American grand narrative was planted in romance. Gordon Wood, “Conspiracy and the Paranoid Style: Causality and Deceit in the Eighteenth Century,” *William and Mary Quarterly*, 39 (1982): 401–41.

²⁹ There is an excellent discussion of Beard’s problems with the economic interpretation of history in Ernst A. Breisach, *American Progressive History: An Experiment in Modernization* (Chicago, 1993), chap. 9. The gap between material cause and ideal consequence occurred on every level of his analysis. In *Rise*, the Beards tried to show that “thought and the materials of life evolve together,” but they could say only that this joint progress occurred “in some mysterious way.” Charles Beard and Mary Beard, *The Rise of American Civilization* (New York, 1927), 1: xi. To a group of his colleagues, Charles Beard admitted that he was “baffled about the implications of the economic process itself.” Economic development seemed part of “this enormous thermodynamic process that seems to constitute the substance of the universe,” and he was not sure whether “all our talking and running up and down and politics” could affect it at all. Social Science Research Council, Hanover Conference, 1926, transcript, 500–01 (Charles E. Merriam Papers, University of Chicago, Special Collections), quoted in Ross, *Origins of American Social Science*, 460.

³⁰ Cott shows that Mary Beard’s wide concept of civilization was a major factor in this project; in addition, the Beards claimed Voltaire and Buckle as exemplars. Cott, *Woman Making History*, 28–29; Beard and Beard, *Rise*, xv.

³¹ Note the end of the first paragraph of the book, where the Beards are justifying the attempt to write a complete history of American civilization. (They start off with a formal language appropriate to their serious subject and its historical pedigree.) “The history of a civilization may symbolize a certain coming to maturity in that civilization itself. What adult with any claim to ripeness of spirit would admit belonging merely to one category of history—[then descend to the present in a provocatively plebeian language] as warrior, politician, money-getter, novelist, sportsman, mortician, journalist, husband, wife, father, or mother—and aspiring to nothing more? [followed by the immediate ascent to biblical metaphor] When the dust of the earth became conscious of the dust, a transformation began to take place in the face of the earth.” Beard and Beard, *Rise*, vii.

to the popular audience they wrote for. Judging by their large sales, their audience was indeed still torn, as they themselves were, between ironic doubts and mythic hopes. At the end, they gave a resounding affirmative answer to the question of progress, but they never silenced the conditional mood. "If so," reads the last line of the book, the uncertainty rushing headlong into romance, "it is the dawn, not the dusk, of the gods."³² Indeed, within a few years, the fears that *Rise* barely contained forced the Beards into a renewed effort to shore up the American project. They took up again the nineteenth-century effort to expel the bad causes from American territory, by assigning destructive economic forces to the world outside the United States.

WHEN THE POSTWAR/COLD WAR generation of historians took up their pens, they occupied a very different historical situation from that of the Beards. During the decade after World War II, the country was experiencing the "American Moment" of the "American Century"; the United States seemed already to stand at the summit of world power and already to embody the values its exceptionalist history promised.³³ It was a moment that seemed ripe for the unabashed reassertion of the American-centered grand narrative.

On one level, that is what happened. David Potter's *People of Plenty: Economic Abundance and the American Character* (1954) is the standard example. Potter chose the topic of national character for a series of public lectures, and his book reproduced the sense of achieved American triumph widespread in the postwar decade. He took for granted the distinctiveness of "the American, this 'new man,' as Crèvecoeur called him" and the position of the United States as the richest and most powerful country in the world. Indeed, Potter hardly bothered to give his readers any evidence of abundance except some cursory postwar statistics or of distinctiveness except some scattered reports from European travelers. The national character was presented as inclusive and unchanging. The ahistorical postwar social science that he used, with its emphasis on static systems and on culture-and-personality as the structural element in society, reinforced the story of a distinctive American identity simultaneously achieved at its founding and realized in the present.³⁴

In the face of the Cold War abroad and McCarthyism at home, however, many of the postwar historians found the triumph less than perfect.³⁵ Some of the leading architects of American historiography such as Richard Hofstadter and Louis Hartz had been influenced by Marxism to think of the United States as a monolithically liberal capitalist society in which Beard's "people" and "interests"

³² Beard and Beard, *Rise*, 2: 800.

³³ Edward A. Purcell, Jr., *The Crisis of Democratic Theory: Scientific Naturalism and the Problem of Value* (Lexington, Ky., 1973), pt. 4.

³⁴ David M. Potter, *People of Plenty: Economic Abundance and the American Character* (Chicago, 1954), esp. vii; Fred Matthews, "Social Scientists and the Culture Concept, 1930–1950; The Conflict between Processual and Structural Approaches," *Sociological Theory*, 7 (1984): 87–101.

³⁵ For a view of postwar/Cold War historiography as more complacent and monolithic than I will suggest here, see Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, 1988), pt. 3.

were alike moved by acquisitiveness. While Marxism thus reinforced the new emphasis on liberal consensus, it also added to the residue of irony left by Beard.³⁶ Even more important, the dominant position of the United States in the world quickly revealed the limits of its power and, what was more troubling for the inherited grand narrative, the limits of its example. An external center of reactionary power had been the “other” against which American exceptionalism was originally formed. External enemies, even with ramparts within, had been overcome before. However, the possibility that America’s unique consensus had disabled the country from effectively playing its leading role in the world—a possibility soon reinforced by widespread criticism of the United States in Europe and what was to become the “Third World”—threatened the universal consummation of the American grand narrative.

What was strikingly novel about postwar/Cold War historiography was its construction of American failings as inevitable aspects of success. American virtues necessarily bred their own vices. The exceptionalist success of the United States as an egalitarian democracy had led to a harmonious pluralist order, but this very success and the consequent absence of real ideological conflict and debate had led to inflated expectations and a conformist, absolutist mentality. American failings were thus the product of irrational attitudes, rather than a result of the economic and social realities of American life. It was the false hope of absolute equality that was the major problem, Potter said, not the imperfect realization of equal opportunity. Similarly, he thought the most troubling aspect of American capitalism for the future was not its socioeconomic consequences but the culture of advertising it had created, a pervasive and irresponsible manipulation of individual desires.³⁷ False conceptions were an even more serious problem in relations abroad. According to Hartz, the chief danger bred of liberal consensus was Americans’ inability to understand foreign countries or tolerate their radicalisms, thus disabling the United States from playing an effective role in the world.³⁸ These historians did not doubt the unique character of the United States as the most free, most democratic, most consensual country in the world; however, they linked to that fortunate success unfortunate cultural consequences.

The analysis of the American mind as monolithic, prey to false ideologies, conformist intolerance, and covert manipulation, thus came perilously close to reproducing the defining characteristics of foreign evil: totalitarianism. As Wilfred McClay has shown, a host of liberal thinkers in the 1950s from Erich Fromm to David Riesman were finding a “soft” totalitarianism in the United States parallel to the “hard” totalitarianism attributed to Nazi Germany and the Soviet Union. Rooted in the critique of an atomized mass society that had originated with conservative thinkers after the French Revolution, including Tocqueville, the

³⁶ Richard Hofstadter, “The Importance of Comity in American History,” *Columbia University Forum*, 8 (1970): 9. For Hofstadter and others influenced by Marxism, the sense in the late 1930s that both socialist and liberal narratives of progress had ended in historical impasse also prepared the way for the postwar sense of irony and of stasis in time. See Howard Brick, *Daniel Bell and the Decline of Intellectual Radicalism: Social Theory and Political Reconciliation in the 1940s* (Madison, Wis., 1986), chap. 1.

³⁷ Potter, *People of Plenty*, 108–09, 139, chap. 8.

³⁸ Louis Hartz, *The Liberal Tradition in America* (New York, 1955).

theory was revived by the Frankfurt critics of modern society and culture, many of whom had emigrated to the United States, and then taken up by American intellectuals.³⁹ Reinhold Niebuhr, using a Christian idiom, influentially formulated this paradoxical situation as “the irony of American history” in 1952. “The evils against which we contend are frequently the fruit of illusions which are similar to our own.”⁴⁰ The term “totalitarianism” was not applied to the irrational conformism critics found in the United States; indeed, American society, with its pluralism and individual freedom, was declared its opposite. But the analyses of American conformism and foreign totalitarianism, with their common sources and common resonances, had disturbing implications.

The likeness was not lost on American historians. Hartz felt the need to differentiate America’s “bizarre fulfillment of liberalism” from Russia’s socialism, “twisted alike . . . by laws of ‘combined development.’”⁴¹ C. Vann Woodward, in “The Irony of Southern History” (1953), took up Niebuhr’s lead and drew out its implications. “Our opposite numbers in the world crisis, the Russian Communists, are bred on illusions of innocence and virtue that parallel our own with ironic fidelity, even though they are of very different origin and have been used to disguise (perhaps even from themselves) what seems to us much greater guilt of oppression and cruelty.” It was this “ironic plight of modern America” that led Woodward to urge an “ironic interpretation of history.”⁴²

Woodward appears to suggest not merely a rhetoric of irony but a plot line for the American story in which success has issued in destructive illusions. Technically, this is the plot of satire, a morally chastened story where good and evil are not what they seem, where the moral certainty as well as the redemption or reconciliation promised by romance are denied.⁴³ His language also suggests, however, a less destabilizing alternative. Irony, Woodward wrote, would require historians “able to appreciate both elements in the incongruity that go to make up the ironic situation, both the virtue and the vice to which pretensions of virtue lead.”⁴⁴ The hero flawed by hubris is the stuff of tragedy, a story of failure but of romance reversed rather than canceled. It was the tragic history of the Jeffersonian slave South, with its moral failures and physical defeats, that Woodward counted on to produce historians capable of writing ironic history. Tragedy was still cordoned off in the South from the story of American success, and Woodward held out the hope that national irony would become neither tragedy nor satire. But the evils that previous historians had attributed to Europe or counted on

³⁹ This connection is drawn by Wilfred M. McClay in *The Masterless: Self and Society in Modern America* (Chapel Hill, N.C., 1994), chaps. 6–7, a superb study of the symbiotic relationship between autonomous individualism and social absorption in the nineteenth and twentieth centuries. For a contemporary discussion of the problematic relationship between “soft” and “hard” totalitarianism in the context of Russian history and Foucauldian analysis, see Laura Engelstein, “Combined Underdevelopment: Discipline and the Law in Imperial and Soviet Russia,” *AHR*, 98 (April 1993): 338–54.

⁴⁰ Reinhold Niebuhr, *The Irony of American History* (New York, 1952), 16.

⁴¹ Hartz, *Liberal Tradition*, 308.

⁴² C. Vann Woodward, “The Irony of Southern History,” in *The Burden of Southern History* (1960; New York, 1961), 172–73.

⁴³ White, *Metahistory*, 8–10.

⁴⁴ Woodward, “Irony of Southern History,” 173.

American circumstances to transform or defeat were now seen to be inevitably, ironically linked to the essential features of American success.

The moment of postwar/Cold War triumphalism thus uncovered the threat of a monolithic social order at the heart of American modernity, the threat—I will argue shortly—that has shaped American historiography ever since. The triumphal moment equally marked the beginning of a sense of disorientation in historical time that is still with us. The dark side of the American triumph cast a pall over the future. “As practiced by mature minds,” Hofstadter argued, “history forces us to be aware not only of complexity but of defeat and failure: it tends to deny that high sense of expectation, that hope of ultimate and glorious triumph, that sustains good combatants.”⁴⁵ Although Hofstadter was attacking utopian politics, the “hope of ultimate and glorious triumph” is precisely what had animated the American romance since its origins. An irony deep enough to dash romantic hopes left American historians in an unfamiliar landscape.

The demotion of narrative was one token of that disorientation. David Potter’s book was written around analytic topics rather than chronological development, with only scanty, occasional looks backward for the purpose of comparison, despite his assertion that historians’ unique contribution to the topic was their understanding of historical change. The grand narrative of American triumph that was partly discussed, partly implied was reproduced neither in narrative form as it had been by Bancroft and Beard nor by the standard monographs that carved out small parts of the larger story. *People of Plenty* was what Hofstadter called “the new genre of analytical history . . . part narrative, part personal essay, part systematic empirical inquiry, part speculative philosophy.”⁴⁶ The analytical style is framed by the historian’s argument; the chronological time of its story can be interrupted or obscured to fit the purposes of argument. Expressing the postwar/Cold War sense of both historical closure and historical uncertainty in time, it allowed historians to assume what features they needed of the American grand narrative without having to reconstruct the whole in a form that eschewed romantic fulfillment.

Analytical history also expressed the heightened uncertainties of epistemological skepticism. One major source of analytical history was the desire to use social science theory more systematically in the writing of history, a desire that grew from historians’ doubts of their ability to know the narrative shape of history. Beard’s perplexity over whether the grand narrative of history was a progressive one became, a few years later, doubt whether the historian could ever know the shape of history. Both of these doubts were given more corrosive expression by Carl Becker’s epistemological essays and his *Heavenly City of the Eighteenth-Century Philosophers* (1932), which cast the idea of progress as a doomed attempt to cling to the medieval Christian hope of salvation in a world without God. On the issue of historical knowledge, much of the profession took a compromise position, concluding that certainty was possible with regard to facts but that interpretation

⁴⁵ Hofstadter, “Importance of Comity,” 13.

⁴⁶ Richard Hofstadter, “History and Sociology in the United States,” in *Sociology and History: Methods*, Hofstadter and Seymour Martin Lipset, eds. (New York, 1968), 18.

was necessarily subjective.⁴⁷ Recognizing that history was at bottom an imaginative “representation of the human situation,” Hofstadter turned to social science concepts as a way of enriching historiography. Potter, by contrast, believed that social science theory would remedy “the historian’s lack of systematic procedure in the practice of generalization,” making history more scientific. Whether it was meant to embrace or avoid the instability of interpretation, the social science strategy made visible, in the analytic style, historians’ uncertainty about their ability to know the narrative shape of history.⁴⁸

It would be a mistake, however, to overemphasize the ironic retreat from romance among historians of the United States in the 1950s. The long-nourished hopes of the American-centered grand narrative were not so easily banished from American historiography. For some historians, triumphalism was unalloyed.⁴⁹ Even for the ironists, while American success had taken a satirical turn and its connection to a larger narrative of world progress was still in doubt, the master narrative of American uniqueness remained intact. If grand narrative now had to be a matter of faith or hope, for most historians, hope remained. At a symposium on Becker’s *Heavenly City* in 1956, more participants than not defended the possibility of a chastened view of progress.⁵⁰ Moreover, uncertainty could be mitigated by the theories of social science that inscribed the American ideal as natural process. Modernization theory was particularly important, for it laid out the laws of development that would transform the rest of the world into a likeness of the United States, hence dispelling the fear of American failure as exemplar to the world.⁵¹ Nonetheless, by abandoning romance at the American center of the grand narrative, a leading group of postwar/Cold War historians loosened its grip on the historical imagination. This set in motion questions, anxieties, and hopes that have been producing consequences ever since.

IRONIC HISTORY invited the students of the postwar/Cold War historians to ask questions about things their mentors had taken for granted, specifically the pluralist social-cultural consensus.⁵² The opening to social science led some historians to urge new methods, arguing that it was scientific method that produced reliable generalizations, not borrowed concepts.⁵³ The achievements

⁴⁷ On Beard’s and Becker’s relativism in the profession, see Ian Tyrrell, *The Absent Marx: Class Analysis and Liberal History in Twentieth-Century America* (New York, 1986), 22–23, 36–39, 93–95; and Novick, *That Noble Dream*, pt. 2; on Becker, Carl L. Becker, *The Heavenly City of the Eighteenth Century Philosophers* (New Haven, Conn., 1932); Burleigh Taylor Wilkins, *Carl Becker: A Biographical Study in American Intellectual History* (Cambridge, Mass., 1961).

⁴⁸ Richard Hofstadter, “History and the Social Sciences,” in *The Varieties of History: From Voltaire to the Present*, Fritz Stern, ed. (New York, 1956), 370; Potter, *People of Plenty*, xii.

⁴⁹ One example is Daniel J. Boorstin, *The Americans*, 3 vols. (New York, 1958–73).

⁵⁰ Raymond O. Rockwood, ed., *Carl Becker’s Heavenly City Revisited* (Ithaca, N.Y., 1958).

⁵¹ Daniel Lerner, James S. Coleman, and Ronald P. Dore, “Modernization,” *International Encyclopedia of the Social Sciences* (New York, 1968), 10: 386–409.

⁵² See, for example, Stephan Thernstrom, *Poverty and Progress: Social Mobility in a Nineteenth Century City* (Cambridge, Mass., 1964). His attempt to put the “American legend” (p. 1) to empirical test follows logically from the way Potter’s cohort had analytically separated the myth of social mobility from the reality and questioned the former without questioning the latter.

⁵³ The methodologically oriented social history encompassed a wide range of historical work, from

of European historiography, particularly the new social-cultural history of the *Annales* in France and the English historians around *Past and Present* and the History Workshop also influenced American historians.⁵⁴ At the same time, the composition of the profession changed as the democratization of higher education allowed men and—after the mid-1960s—women from a wider spectrum of American society to become historians.⁵⁵ These factors were already producing a new social history when the political conflict of the 1960s gave the movement new energy and direction. The concatenation of the civil rights movement, the war in Vietnam, youth rebellion, and the women's movement decisively ended the "American Moment" and the premonitions of triumph and disaster it had locked in ironic embrace. What has been called the "New Left" drew into political debate and then into the historical profession a range of radical views, based in liberal democratic, populist, Marxist, and feminist traditions as well as in contemporary radical movements.

The methodologically oriented "history from the bottom up" was now joined and increasingly overshadowed by a broader and more disruptive reaction to postwar/Cold War historiography: a social-cultural history that sought the history of the "inarticulate," the working class, racial minorities, and women—those who had been marginalized in American history and left out of its historiography.⁵⁶ Social-cultural history shifted the central subject of American history from the American polity itself—its distinctive identity, growing freedom, or democracy—to these hitherto marginalized groups. It is noteworthy that in the 1890s, when the first tentative steps were taken toward social history, Sloane recognized and feared the possibility of this shift: "We must not go too far in yielding to a popular clamor, nor admit that the weight of the individual in modern life entitles his occupations and beliefs to more than a certain moderate share in the story of the organism to which he belongs." Sloane's defense—"Keep the emphasis on the state"—was now for the first time fully breached.⁵⁷

The democratic thrust of the new history was very different from the elitest critique of the popular mind that had occupied the 1950s historians, but the totalitarian potential of American society remained central to both generations. The target of the political energies let loose in the 1960s was the monolithically

demography and econometrics to studies of crowd behavior, voting patterns, and organizational theory. For an early sampling, see Robert P. Swierenga, *Quantification in American History: Theory and Research* (New York, 1970); for more recent work, the journal of the Social Science History Association, *Social Science History*.

⁵⁴ Lynn Hunt, ed., *The New Cultural History* (Berkeley, Calif., 1989), introduction and pt. 1.

⁵⁵ Novick, *That Noble Dream*, 362–67, 470.

⁵⁶ Novick, *That Noble Dream*, chap. 13.

⁵⁷ Sloane, "History and Democracy," 8. Although the progressive historians had sought a wider view of social life and Mary Beard had made *Rise* unusually inclusive of women, the state remained at the center of their focus, and the story of "the people" was still told from the top down and in very general terms. Until the civil rights and women's movements began to take effect, the ironic postwar/Cold War historians paid virtually no attention to women, nor to African Americans except in the context of slavery and the South. However, David Potter made good his failure to consider women in *People of Plenty* in an essay, "American Women and the American Character" (1962), *History and American Society: Essays of David M. Potter*, Don E. Fehrenbacher, ed. (New York, 1973), 277–303, although he did not think to ask whether African Americans shared that national character or how they might have contributed to it.

liberal society constructed by the Cold War historians. Post-sixties historians turned the mass-society critique not only against the popular myths and false desires that consensus had ironically produced but against the consensus itself. They argued that liberal consensus was not voluntary; it was coerced by the processes of capitalism, bureaucratic rationalization, mass culture, and their ruling elites. Pluralist social and political institutions were not genuinely pluralist but reinforced the status quo.⁵⁸ The social historians' fear of a coercive consensus that silenced difference deepened when the center of political gravity in the United States moved to the right by exploiting a cultural politics of intolerance. Political disillusionment, now reinforced by the decimation of socialist ideals abroad and the collapse of liberal politics in the United States, led social historians to extend the search for means of escape from collective authorities, producing in the 1980s a new cultural history.⁵⁹ In the process, the egalitarian and anti-authoritarian democratic agenda shifted somewhat from an initial emphasis on democratic participation and shared power, inspired by Marxist and social democratic ideals, to a focus on community, identity, and a search for an authentic pluralism.⁶⁰

The most recent components of that cultural history, postmodern literary theories and philosophies, have especially reinforced its anti-authoritarian and pluralist political thrust. Postmodern politics, in the words of John McGowan, "is driven by the simultaneous fear that a monolithic social order shapes contemporary life and hope that a strategy for preserving pluralism (difference) can be found."⁶¹ This is the fear and hope that first became visible among the postwar/Cold War intellectuals, with their fear of hard totalitarianism abroad and soft totalitarianism at home and their hope for American democracy as the preserve of pluralism. Richard Rorty enacts the movement from that moment to postmodernism in his own personal history. The concrete politics he draws from it, what he calls "ethnocentric, bourgeois liberalism," is reminiscent of the anti-Marxist liberalism of that time.⁶² For the 1960s radicals and their heirs on the left, the focus was more on the authoritarian threat of American society than that of the Soviet Union. Authentic communities and individual autonomy were set in opposition against the coercive modern society drawn by such Frankfurt critics

⁵⁸ For the movement of the mass-society critique into the orbit of the New Left in the 1960s, see McClay, *Masterless*, chap. 8; James Miller, *Democracy Is in the Streets* (New York, 1987), pt. 2; "History as Social Criticism: Conversations with Christopher Lasch," interviews by Casey Blake and Christopher Phelps, *Journal of American History*, 80 (1994): 1317–18, 1321. For examples of New Left historical critique, see Christopher Lasch, *The New Radicalism in America (1889–1963): The Intellectual as a Social Type* (New York, 1965); James Weinstein, *The Corporate Ideal in the Liberal State, 1900–1918* (Boston, 1968); William Appleman Williams, *The Contours of American History* (Cleveland, Ohio, 1961).

⁵⁹ For one witness to this disillusionment, see Richard Wightman Fox and T. J. Jackson Lears, *The Power of Culture: Critical Essays in American History* (Chicago, 1993), 3.

⁶⁰ I am indebted to Douglas Rossinow for calling my attention to the fact that the meaning of democracy itself changed as the agenda of post-sixties historiography shifted from the critique of established power to the recovery of social-cultural history.

⁶¹ John McGowan, *Postmodernism and Its Critics* (Ithaca, N.Y., 1991), x.

⁶² L. S. Klepp, "Every Man a Philosopher-King," *New York Times Magazine* (December 2, 1990): 57 and following; Richard Rorty, "Postmodernist Bourgeois Liberalism," *Journal of Philosophy*, 80 (1983): 583–89. It is noteworthy that Rorty continues the postwar/Cold War emphasis on irony but has made it the reigning stance of the private rather than the public sphere. See Richard Rorty, *Contingency, Irony, and Solidarity* (Cambridge, 1989), chap. 4.

as Herbert Marcuse and the Weberian C. Wright Mills, then analyzed in Antonio Gramsci's concept of hegemony and Michel Foucault's histories of modern disciplinary practices and coercive "epistemes."⁶³ Modernization theory as the movement from *Gemeinschaft* to *Gesellschaft* was turned from a story of growing liberal freedom into a narrative of the loss of community.⁶⁴

The post-sixties historians put most of their effort into social-cultural history and the recovery of the diversity of the past. If they shared the fears of the postwar/Cold War era, they were more confident that they had a positive program to combat them. Underneath and outside the limits of consensus, they argued, workers, immigrants, racial minorities, and women had resisted domination and maintained their own group identities. Some of the separate histories of these groups, especially those inspired by Marxism, made the dominant political, social, and economic structures that oppressed them a focus of critical attention.⁶⁵ Some historians, too, discussed the experience and constraints of subordinate groups ironically.⁶⁶ By and large, however, the democratic agenda of the social-cultural historians was anything but ironic. Moved by populist, socialist, and/or feminist political sympathies, they often valorized their subjects' resistance to oppression and sturdy survival or, conversely, their victimization by oppressors.⁶⁷ The romantic tendency that had earlier given mythic shape to the national story of American democracy now animated the stories of the social-cultural groups within it.

Culture was not the only focus of this new history, but it was an important tool in its democratic strategy. Because the social historians' subjects failed to gain major political power and because the critique of oppression sometimes cast these subjects as the helpless and unwitting victims of dominant elites, culture became the site of their indigenous strength. As Lawrence Levine recently noted, "This is what so much of recent historiography is primarily about—not the plight of victims but the culture, the thought, the *lives* of people we have previously neglected." It is about the many kinds of power they exerted, "especially cultural power, the importance of which we are just now beginning to appreciate."⁶⁸ One line of cultural analysis worked to rescue popular culture from the pessimism of

⁶³ In addition to the sources cited above, n. 54, see Hunt, *New Cultural History*, pt. 1.

⁶⁴ This ambivalence was present in the late nineteenth-century theories of historical transformation that were incorporated into modernization theory, though ignored by its proponents in the 1950s and 1960s. See Harry Liebersohn, *Fate and Utopia in German Sociology, 1870–1923* (Cambridge, Mass., 1988). On usage by American historians, see Thomas Bender, *Community and Social Change in America* (1978; Baltimore, Md., 1982); Kenneth Cmiel, "Destiny and Amnesia: The Vision of Modernity in Robert Wiebe's *The Search for Order*," *Reviews in American History*, 21 (1993): 352–68.

⁶⁵ For example, Jeanne Boydston, *Home and Work: Housework, Wages, and the Ideology of Labor in the Early Republic* (New York, 1990); David M. Gordon, Richard Edwards, and Michael Reich, *Segmented Work, Divided Workers: The Historical Transformation of Labor in the United States* (Cambridge, 1982); J. Carroll Moody and Alice Kessler-Harris, eds., *Perspectives on American Labor History: The Problem of Synthesis* (De Kalb, Ill., 1989).

⁶⁶ For example, Linda K. Kerber, *Women of the Republic: Intellect and Ideology in Revolutionary America* (Chapel Hill, N.C., 1980), ironically balances women's ability to use the opportunities of revolution against their limitations within a conservative political and social order.

⁶⁷ For a representative sampler, see Michael Kammen, ed., *The Past before Us: Contemporary Historical Writing in the United States* (Ithaca, N.Y., 1980), particularly the essay by Peter N. Stearns, "Toward a Wider Vision: Trends in Social History."

⁶⁸ Lawrence W. Levine, "Clio, Canons, and Culture," *Journal of American History*, 93 (1993): 864.

the Frankfurt School on the one hand and the economic determinism of orthodox Marxism on the other. Warren Susman, a pioneer of this analysis, argued that popular culture could be a realm of authentic private values and social resistance for ordinary people.⁶⁹

Another line of analysis drew on the symbolic anthropology of Clifford Geertz and others who depicted culture as the primary realm of integration and meaning in peoples' lives. E. P. Thompson and his American disciples used that holistic theory for their own sub-cultural purposes. In the work of Herbert G. Gutman and other historians of the working class and ethnic and racial groups, culture was the medium that nourished and protected group identities and individual humanity.⁷⁰ In women's history, too, the concept of "separate spheres" opened the way to sub-cultural interpretation that reversed the dominant valences. Carroll Smith-Rosenberg argued that the female sphere of nineteenth-century middle-class women was not a realm of subordination and victimization but one of autonomy and strength. Centered in female intimacy rather than fraternal relations, it carried the same positive value of social solidarity as Thompson's working-class culture.⁷¹ Poststructural literary theories have also figured in this cultural strategy. Many of these theories present language as broken, contradictory, and open to reconstruction by its speakers. The implications of this postmodern sensibility for American historiography are complex, and I will return to them. But, for the time being, I want to emphasize that exposure to linguistic theory in part reinforced the democratic program already under way in social-cultural history. Historians of women, for example, quickly found it useful to discuss the way culture "constructs" gender differences and the way women re-figure those differences in their own lives.⁷²

The social-cultural history of the 1970s and 1980s loosed immense creative energies and quickly produced a rich literature on the history of labor, women, racial and ethnic groups, and popular culture. Accentuating the multiple divisions already caused by specialization, social-cultural history gave rise to an intensified lament over the "fragmentation" of historiography: these separate stories were not being integrated with the results of previous historiography into a single story about the United States or the West. The complaint was usually framed as a problem of synthesis, of putting the many small pieces of American history together, and was sometimes linked to the assumption that a central story would enable professional historians to reach a wider popular audience. What many of the critics seemed to want was not just a single, clear story but a story that reaffirmed an American-centered grand narrative. Although some of the loudest

⁶⁹ Warren I. Susman, *Culture as History: The Transformation of American Society in the Twentieth Century* (New York, 1984).

⁷⁰ Suzanne Desan, "Crowds, Community, and Ritual in the Work of E. P. Thompson and Natalie Davis," in Hunt, *New Cultural History*, 47–71; Ira Berlin, "Introduction: Herbert G. Gutman and the American Working Class," in Herbert G. Gutman, *Power and Culture: Essays on the American Working Class*, Ira Berlin, ed. (New York, 1987), 36–45.

⁷¹ Carroll Smith-Rosenberg, "The Female World of Love and Ritual: Relations between Women in Nineteenth Century America," *Signs*, 1 (1975): 1–29. See also Linda K. Kerber, "Separate Spheres, Female Worlds, Woman's Place: The Rhetoric of Women's History," *Journal of American History*, 75 (1988): 9–39.

⁷² See Terry Eagleton, *Literary Theory: An Introduction* (Minneapolis, Minn., 1983), chap. 4.

complaints came from political conservatives, the call for synthesis came from all parts of the political spectrum, from historians who wanted to reattach the separate stories to a liberal narrative of American democracy as well as those who hoped to move social democratic values to the center of a revised narrative.⁷³ As difficult as it might be for contemporary historians to encompass the immense complexity of current historical literature, the problem is less that of synthesis, I believe, than of narrative coherence, a problem of joining the kind of subjects social-cultural history has constructed to plots embedded in inherited grand narratives.

The romantic strain in social-cultural history works to validate the separate lives of the groups that were hitherto marginalized in American history and historiography. Moved by a combination of democratic egalitarianism and anti-authoritarian individualism, social-cultural historians want to accord unconditional weight to their subjects—a weight that can be heard in Levine's stress on "the culture, the thought, the *lives* of people we have previously neglected."⁷⁴ These historians are determined that those who have, by some standards, lost in life will not lose in historiography. Romance requires, if not victory in every story, a narrative frame validating either victory or loss. It also requires that the moral value of these subjects and their opponents be sharply polarized.

The desire to valorize the subjects of social-cultural history has been both accentuated and made more difficult as historiography has developed over the past two decades. Increasing historical sophistication and democratic sensitivities to each historical subject soon led historians of race, gender, and class to recognize the existence of greater complexity *within* each social-cultural category: racial, ethnic, class, and gender identities sometimes cross-cut and sometimes fused with each other. The postmodern valorization not simply of differences, but of difference, with its fear of essentializing any category, has strengthened that recognition. As the political theorist Kirstie McClure has noted, "Where social subjects are complexly constituted not only through categories of gender, but of race and sexuality, ethnicity and class, and perhaps of religion and nationality as well, a position of privilege within one frame may be simultaneously and contradictorily constructed within a position of oppression within another."⁷⁵ The result is to push historians to an increasingly atomized level of analysis while they try to maintain the conceptual and moral integrity of social-cultural groups.

At the same time, grand narrative has remained, if in elusive forms. Contemporary American historians inherited a variety of grand narratives—a Marxist narrative of socialist and working-class triumph, a liberal narrative of democratic capitalist progress, a social democratic narrative of capitalist transformation that

⁷³ For the sources of that complaint, as well as an exemplar of it, see Novick, *That Noble Dream*, pt. 4.

⁷⁴ See also Alice Kessler-Harris, "A New Agenda for American Labor History: A Gendered Analysis and the Question of Class," in Moody and Kessler-Harris, *Perspectives on American Labor History*, where she remarks that historians of the working class have been reluctant to abandon the "heroism of ordinary lives [and] everyday resistance to capitalism," 220–23.

⁷⁵ Kirstie McClure, "On the Subject of Rights: Pluralism, Plurality, and Political Identity," in *Dimensions of Radical Democracy: Pluralism, Citizenship, Community*, Chantal Mouffe, ed. (London, 1992), 28.

lies between them, and American-centered variants of all three. These narratives move forward within history, powered by mythic hope, to a happy ending. Given the epistemological doubts raised about interpretation since the days of Beard, these grand narratives have seldom been fully articulated, but historians have continued to use them, consciously and unconsciously, as historico-political frames in constructing their narratives.

Even as political hope, however, grand narrative has become more elusive in recent decades. It hardly needs repeating that historical events, political disillusionment, and historiographical sophistication have taken a heavy toll on the plausibility of any of them. The toll has been heaviest on the Marxist and social democratic left, where these factors have redirected attention to the satisfactions as well as oppressions of the market economy. In Richard Fox's and T. J. Jackson Lears' new collection of essays in cultural history, for example, which of these mixed valuations is dominant and which the harbinger of the future is left very much in doubt.⁷⁶

Liberal grand narrative has suffered less erosion. The large reserve of liberal and exceptionalist sympathies in American society has kept it alive in American historiography. It is implicit in many of the monographic stories of limited liberal progress that continue to be told, stories of modernization, market expansion, or growing social and political democracy in the United States. Since 1989 and the defeat of communism, there is even the possibility of a return to triumphalism, as the United States once again can be imagined to assume its original world-historical role. Still, disillusion with contemporary liberal politics, growing economic inequality, and the inescapable diversity of the world have made even a liberal happy ending seem more distant. The result for both liberal and leftist historians is that, even as a regulative principle, the force of grand narrative has been unsteady.

THE RHETORICAL DIFFICULTIES created by the validation of all the subjects of history and the elusiveness of grand narrative are evident when we look at some of the grand narratives that have not been written.

Herbert Gutman, one of the leading social historians working in the Marxist tradition, issued one of the first calls for synthesis in 1981. He imagined "a new synthesis . . . that incorporates and then transcends the new history," bringing the new understanding of blacks, the working class, and women to bear on "American history." Yet Gutman did not go on to specifics but to poetics. "T. S. Eliot phrased it well in *The Waste Land*. It was almost as if he were predicting the condition of American history after 1950.

'What are the roots that clutch, what branches grow
Out of this stony rubbish? Son of man
You cannot say, or guess, for you know only
A heap of broken images.'

⁷⁶ Fox and Lears, *Power of Culture*.

We need to go beyond the heap of broken historical images to live more decently and humanely in the late twentieth century.”⁷⁷ Gutman’s invocation of Eliot’s Christian imagery of redemption is exactly right, for it recalls the mythic basis of the American and the Marxist grand narratives that he hoped to join. In his own work on labor history, however, he could not do so.

A comparison of Gutman’s long essay, “Work, Culture, and Society in Industrializing America,” the closest he came to an overview of American labor history, with the work on which he modeled it, Thompson’s *Making of the English Working Class*, is revealing. Although Gutman’s essay can hardly be expected to match Thompson’s rich volume, both of them stop their story before the appearance of strong working-class organization—Thompson at the Chartists, Gutman at World War I—leaving them considerable choice in how to construct their narratives. Despite the ambiguous fate of labor in modern Britain and the defeat of the artisans he chronicled, Thompson wrote a resounding romance around the figure of the heroic artisan. He did so by identifying the artisans with a working-class culture that was not only the persisting source of worker’s strength but the seedbed of the class consciousness and labor organization that followed. And if the twentieth-century fate of that consciousness and organization was still in doubt, “Causes which were lost in England might, in Asia or Africa, yet be won.” He did not judge his historical subjects by the standards of “subsequent evolution” but by his hopes for a happy ending in a Marxist grand narrative of history.⁷⁸ In contrast, Gutman’s story of the American working class, peopled by waves of immigrants, is tentative and ambiguous. Whether ethnic working-class cultures that persist in the nineteenth century can survive or give rise to class consciousness in the twentieth is barely broached and is left uncertain. At the end, he evocatively notes some remnants of working-class culture hidden in the present, but implicit in these stray shards is the larger loss they tell.⁷⁹ Gutman cannot quite summon the Marxist hope that would turn the ambiguous story of the American working class into romance.⁸⁰

A grand narrative of progressive struggle, however, appears within closer reach. Two recent books by social-cultural historians of the Left suggest the possibilities and difficulties of such a narrative. David Roediger’s recent study of the role played by white male racism in the formation of the antebellum working class and Linda Gordon’s study of how the maternalist assumptions of women reformers deformed the modern welfare system are landmarks of the current complex sensibility of social-cultural history. Race, class, and gender work with and against each other in these stories in ways that seriously compromise the heroism of their subjects. However, both Roediger and Gordon place their stories within a com-

⁷⁷ Herbert G. Gutman, “The Missing Synthesis: What Ever Happened to History?” *Nation* (November 21, 1981): 554.

⁷⁸ E. P. Thompson, *The Making of the English Working Class* (1963; rpt. edn., New York, 1966), 13.

⁷⁹ Herbert G. Gutman, “Work, Culture, and Society in Industrializing America, 1815–1919,” in *Work, Culture, and Society in Industrializing America: Essays in American Working-Class and Social History* (New York, 1976), 3–78.

⁸⁰ See also the revealing report of a conference on labor history that unsuccessfully sought to promote synthesis of the field: “Though they hung like stagnant air, over the conference, we never directly confronted the questions raised by a pessimistic view of the contemporary working class and organized labor’s possibilities.” Kessler-Harris, “New Agenda,” 221.

mitment to progressive change. The Marxist or social democratic outlines of that larger story and its idealized endpoint are never specified. Gordon, for example, urges only that we “continue” on the project of social justice. Learning from past mistakes “does not promise that knowledge will transform, only that lack of it dooms us.” These unspecified frames nonetheless allow them to present their flawed tales—in Roediger’s case, a deliberately tragic tale—as episodes in a larger story of struggle that at least holds out the possibility of progress.⁸¹

The difficulties they would have in expanding the focus of their stories is palpable, however, in the pressure they feel to preserve the conceptual and moral integrity that remains to women and the working class. Gordon refuses, for example, to use a rhetoric of irony where her reader expects it, at the moral gap between her subjects’ actual behavior and the behavior she desires of them. Even more revealing is an essay by Roediger subsequent to his book that takes to heart the growing demands placed on him by his reviewers and labor historiography. The labor historian, he agrees, must deal with all the concrete intersections of race, class, and gender, not merely the intersection of white/male/working class:

Once we acknowledge that the class identity of, say, an African-American woman worker is influenced not only by her own race and gender identities but also by social relationships with, say, Chinese males (and vice versa), we see the practical difficulties associated with treating race, class, and gender in what Tera Hunter brilliantly terms their “simultaneity.”

Roediger nonetheless accepts the difficulty: the story of the working class must be told from all the multiple points of view within it. Yet he still holds out hope for “a synthesis which makes the role of the whole working class central to the drama of U.S. history.”⁸² Even with all the rhetorical resources of modernist literature, it is difficult to see how the space can be closed between such a relentless drive for the integrity of each historical subject and the integrative demands of a narrative frame.

If we turn now to some liberal grand narratives that have not been written, the same difficulty is at work. Thomas Bender has offered a liberal plot that would synthesize the results of the new social history in a single story: the making of “public culture” in the United States. The choice of public culture links Bender’s proposed synthesis to the American-centered grand narrative with its story of American democracy, as well as to contemporary efforts, such as that of Jürgen Habermas, to reconstitute a social democratic liberalism. Bender proposes to cast

⁸¹ David Roediger, *The Wages of Whiteness: Race and the Making of the American Working Class* (London, 1991), 13, 176–81; Linda Gordon, *Pitied But Not Entitled: Single Mothers and the History of Welfare, 1890–1935* (New York, 1994), chap. 10 and 305–06. Their ability to sustain a progressive, if unspecified, grand narrative may be related to the progress they see in their specialized fields of history. It is noteworthy that both of them write with satisfaction about the growing complexity of their fields and optimistically about the historiographical future, although only Gordon links that optimism to a thriving radical movement outside the academy. David Roediger, “Race and the Working-Class Past in the United States: Multiple Identities and the Future of Labor History,” *International Review of Social History*, 38 (1993), Supplement, 127–43; Linda Gordon, “How Far We’ve Come,” *Radical Historians Newsletter*, no. 71 (November 1994): 1, 13.

⁸² Roediger, “Race and the Working-Class Past,” 133, 143. See also the daunting advice to achieve synthesis in labor history by integrating it with the history of women and/or gender analysis, by Alice Kessler-Harris, “New Agenda,” and Mari Jo Buhle, “Gender and Labor History,” in Moody and Kessler-Harris, *Perspectives on American Labor History*, 79, 224–32.

the public realm not as the seat of a dominant consensus but as “an arena for the play of cultures and interests in society and the product of that play.”⁸³ He seems to have had in mind a liberal comedy that would give equal weight to the shifting centrifugal and centripetal forces in American history. But a story whose chief subject is the public culture would inevitably sacrifice its social-cultural components to the public result. Bender’s many critics want to accord unconditional value to individual and communal voices irrespective of their participation or fate in a common story. As Roy Rosenweig asked, “In whose voice” is the story to be told and in what “tone”? On what basis is the story mapped as a single “core” and a necessarily subordinate “periphery”?⁸⁴ The problem is not “synthesis” but plotting a liberal narrative that leaves the subjects of social history undiminished.

While Bender persists in his effort,⁸⁵ John Higham, sensing the impasse, has recently suggested abandoning altogether the effort to include the new social and cultural history in a centered national narrative. Instead, he divides national historiography in two, assigning social-cultural history to transnational/international history, where its causal chains and cultural configurations can be followed across national borders, and a national narrative that will focus on the unifying themes of American history. The subjects of social-cultural history would retain their integrity as workers or women in a transnational story but would become participants in nationalizing forces in the national story. Even as it opens up national historiography to a more cosmopolitan view, Higham’s move recalls the exceptionalist strategy of expelling abroad those elements that disrupt the American center of the grand narrative.⁸⁶

Another attempt to revive the unique national story that re-centers it away from the subjects of social-cultural history is Gordon S. Wood’s book *The Radicalism of the American Revolution* (1992). Although it parallels in many ways J. Franklin Jameson’s modest *The American Revolution as a Social Movement*, the difference in tone and plot is striking. Jameson engaged in a perspicuous and balanced discussion of the “levelling” democratic effects of the revolution, effects that had not been intended by its elite leaders. In these democratizing social consequences, he said, the American Revolution was, for all its differences from the French Revolution, like it and like other ancient and modern revolutions.⁸⁷ Wood has transformed Jameson’s sober comedy of the American and Western movement from less to more democracy into a triumphal romance of how America achieved a unique identity: as the subtitle proclaims, “How a Revolution Transformed a Monarchical Society into a Democratic One Unlike Any That Had Ever Existed,” or a bit later, how Americans became “almost over night, the most liberal, the most democratic, the most commercially minded, and the most modern people in

⁸³ Thomas Bender, “Wholes and Parts: The Need for Synthesis in American History,” *Journal of American History*, 73 (1986): 127, 131.

⁸⁴ “A Round Table: Synthesis in American History,” *Journal of American History*, 74 (1987): 118–19.

⁸⁵ Thomas Bender, “‘Venturesome and Cautious’: American History in the 1990s,” *Journal of American History*, 81 (1994), repeats the call for a synthesis around “the public culture as the field where contingent and multiple identities, rooted partly in homogeneous and *gemeinschaftlich* groups, are formed into distinctly American identities” (p. 995).

⁸⁶ John Higham, “The Future of American History,” *Journal of American History*, 80 (1994): 1289–1309.

⁸⁷ J. Franklin Jameson, *The American Revolution Considered as a Social Movement* (1926; Boston, 1956).

the world." There is a thread of irony in the plot, one that very much resembles the Beards'. The gentlemen that lead the revolution do not foresee or want the democracy that emerges from it, a democracy of "common people with their common interests in making money and getting ahead." But, like the Beards, Wood retrieves romance from its jaws by deploying a romantically inflated rhetoric.⁸⁸

Wood can tell his story as romance because he focuses, as he says in an acrimonious exchange with his social-cultural history critics, on the new liberty of middling white men rather than on the condition of the total population. In doing so, he asserts, he has "placed the proportions of the story in accord with those of the eighteenth-century, instead of those of our present."⁸⁹ But surely there are no "proportions . . . of the eighteenth-century" apart from our historical interpretation of them. Wood's context of interpretation is clear:

That all white males were equal in 1776 was something revolutionary and new under the sun. In my book, I wanted to get that point clear; for once the claim of equality by all white males was established in the eighteenth century (no mean feat since it took a few thousand years of Western history to accomplish), then the other claims to equality could follow and, relative to the total span of Western history, although not to our brief American past, follow rather rapidly.⁹⁰

Wood's story is framed by an American-centered grand narrative of Western liberal progress, a narrative that makes the American Revolution and American democracy central to the achievement of an ever more inclusive liberal democracy in the world. Far from being rooted in the past, liberal grand narrative judges the past by its progressive trend: the *real* story of the late eighteenth century is the story that is the harbinger of future liberalization. Wood is no less present-minded than his critics, whose feminist, neo-Marxist, or social democratic grand narratives show the roots in the past of still-binding disabilities.

A very different alternative for authors who seek a central story of liberal democracy is to fully incorporate into that story the *exclusion* of these subjects. Each of these subjects presents different possibilities and difficulties. The relative success of feminism over the last generation suggests that a progressive liberal history might be able to include women's history. The latest synthetic history of women in America, for example, tells the story of how American women, drawing on "our nation's distinctive democratic political culture," have progressively overcome obstacles "to claim for themselves the status of full participants in the construction of the American dream." In Sara Evans's story, exclusions and setbacks only heighten the romantic plot of American liberal history.⁹¹

Race, as the most intractable problem of American history, presents a more

⁸⁸ Gordon S. Wood, *The Radicalism of the American Revolution* (New York, 1991), 6–7, 369. It is noteworthy that one of the things Wood's critics in the *William and Mary Quarterly* especially disliked was the book's romantically inflated rhetoric, a point made as well by Joyce Appleby, who also writes within a liberal grand narrative: "Forum: How Revolutionary Was the Revolution? A Discussion of Gordon S. Wood's *The Radicalism of the American Revolution*," *William and Mary Quarterly*, 11 (1994): 677–716, esp. 679, 681.

⁸⁹ "Forum: How Revolutionary Was the Revolution?" 706.

⁹⁰ "Forum: How Revolutionary Was the Revolution?" 707.

⁹¹ Sara M. Evans, *Born for Liberty: A History of Women in America* (New York, 1989), 3, 314.

difficult case. In the thoughtful and challenging work of Nathan Huggins, an American narrative that adequately includes the experience of African Americans shatters the traditional liberal frame. Huggins proposed a narrative of American history that fully faced “the inescapable paradox: a free nation, inspired by the Rights of Man, having to rest on slavery . . . [S]lavery and freedom, white and black, are joined at the hip.”⁹² As the metaphor of monstrosity suggests, however, it would be difficult to prevent such a story of ironic paradox from becoming a savage satire. In *Black Odyssey: The African-American Ordeal in Slavery*, Huggins argued that an American history that fully includes slavery demands something more than the rhetoric of liberal irony. By “making slavery crucial to its social and economical development, the United States became something other than a free society.” Or again, “Those who would be tyrants cannot be called free men.”⁹³ Huggins suggested that some themes can be applied to both whites and blacks, as each seeks to find promise in the new land and each suffers from displacement. But if the American nation can no longer be characterized as free, nor its white men as free men, this joint story, as Levine remarked, would require “a new structural understanding of our history.”⁹⁴

FINALLY, I WANT TO LOOK at the rhetorical possibilities and difficulties created by the most recent contributor to contemporary uncertainty about the larger shape of history, postmodern theory. Postmodern philosophy and linguistic theory deny the ability of the historian to know whether a grand narrative of history exists or not, let alone what its shape might be. What gives postmodern theory its cutting edge is not only epistemological doubt, however, but a politics that judges grand narrative to be a coercive category, one that by its normative inclusive character denies its own fictionality and instability and thereby distorts the creative possibilities of the present and future. As we have seen, historians such as E. P. Thompson, Linda Gordon, Thomas Bender, and Gordon Wood have used grand narrative as a frame that energized and plotted their smaller narratives and connected history to their contemporary political ideals. Used with more or less self-consciousness—and the more self-consciousness the better—grand narrative helped to make the flux of history and its manifold possibilities more coherent. For postmodern theory, however, such coherence is limiting rather than enabling. The problem with current historiography is not the difficulty of reviving an elusive grand narrative or of fitting it to the subjects of social-cultural history. Rather, it is how to write history in a way that disarms grand narrative of its coercive hold on the imagination.

One recent study of the implications of postmodern theory for historiography suggests that historians can reject grand narrative or they can use it as a regulative, heuristic, or ironic principle while calling it into question.⁹⁵ The latter is the

⁹² Nathan Irvin Huggins, quoted in Levine, “Clio, Canons, and Culture,” 866.

⁹³ Nathan Irvin Huggins, *Black Odyssey: The African-American Ordeal in Slavery* (1977; New York, 1990), xi, 245.

⁹⁴ Levine, “Clio, Canons, and Culture,” 866.

⁹⁵ Megill, “‘Grand Narrative’ and the Discipline of History.”

strategy of Rorty in supporting an American-centered liberal narrative of history yet proclaiming it to be based in ethnocentric judgments rather than universal knowledge. Historians have also begun to imagine and construct histories that rhetorically call grand narrative into question. Whether or not such experimental rhetorical strategies are motivated primarily by epistemological concerns, they reflect attempts, informed by postmodern theory, to make historical writing reflect uncertainty about the shape of history. Because so few such works exist, I will not stay wholly within the borders of U.S. history. As we will see, the postmodern questioning or rejection of grand narrative can serve a variety of political purposes.⁹⁶

In a new book on the transformation of the American economy and culture from 1850 to 1940, James Livingston writes from the left, yet does so specifically to “violate the narrative protocol now enforced by the ‘new social history’ and its disciplinary armature, through which the rise of corporate capitalism appears as tragedy, as a betrayal of the ‘democratic promise’ specific to subaltern social movements.”⁹⁷ Instead, he proposes to regard corporate capitalism and its consumer culture as open to constructive historical development and pragmatism, particularly William James’s pragmatism, as the appropriate medium through which to accept and understand its moral and social novelties. In form, the book is a species of Hofstadter’s analytic history but in a postmodern key. What makes it so is, in part, its violation of contemporary disciplinary boundaries, combining economic and intellectual history, social thought and literary fictions; more deeply, it demonstrates the uncertainty of interpretive constructions of historical change. Going beyond the wary openness toward the market of Lears and Fox, Livingston proposes an acceptance that will avoid “exiling ourselves from our own time,” an acceptance by which “the decline of proprietary capitalism loses its pathos, and the triumph of corporate capitalism appears as the first act of an unfinished comedy.”⁹⁸ Grand narrative, rendered problematic rhetorically and by the pragmatism the book explicates, serves a heuristic purpose.

In *City of Dreadful Delight*, Judith R. Walkowitz instead chooses to remain in uncertainty, leaving open the possibility of progress, circularity, or decline. The book is about the narratives of gender and sexuality that circulated in 1880s London, a key moment and location in the larger transformation of capitalist culture and, specifically, of women’s entry into its public spaces. Focused on a

⁹⁶ Two works not discussed in the text are worth noting. Lewis Perry, *Boats against the Current: American Culture between Revolution and Modernity, 1820–1860* (New York, 1993), is a meditation on the way antebellum Americans experienced modernity that uses modernist literary techniques to blur the distinction between past and present, historical actors and the historical author. However, the book aims to show that the experience of modernity was similar in the past and present and does not problematize modernization as a historical category, suggesting an experiment only partially carried through. David Hollinger’s exploration of the possibility of regaining a universalistic perspective on the basis of epistemological uncertainty and multicultural awareness does not deal with the issue of narrative but focuses on the problem of identifying the entities about which narratives can be written. It is suggestive of the ways a liberal historian might construct a narrative around questioning both the commonalities and differences in American/Western history. David A. Hollinger, “How Wide the Circle of the ‘We’? American Intellectuals and the Problem of the Ethnos since World War II,” *AHR*, 98 (April 1993): 317–37.

⁹⁷ James Livingston, *Pragmatism and the Political Economy of Cultural Revolution, 1850–1940* (Chapel Hill, N.C., 1994), xv–xvi.

⁹⁸ Livingston, *Pragmatism and the Political Economy*, 294, xvi.

brief span of years, Walkowitz orders her story in space rather than in time and episodically follows her narratives as they burst into the public press or were reconstituted in different social venues. No effort is made to tell a chronological story; instead of moving through time, we are shown how culture and the material and social worlds constitute each other. We are introduced to the material city, for example, not through an omniscient historian's eye but through the imaginary maps contemporaries drew of it. The reader, like the inhabitants of 1880s London, is embedded in the uncertainties of a brief moment in time and a heterogeneous space.⁹⁹

There are two large stories within which Walkowitz places her book. The minor one for her is Livingston's main narrative of market culture, and Walkowitz shows the market to have "contradictory political effects."¹⁰⁰ It both creates misogynist texts and allows women to create their own texts. Her major narrative is about feminism, and here Walkowitz introduces the problem of change over time by writing an epilogue on the late 1970s. Still, she deliberately refuses to give that larger narrative a shape. Grand narrative as comedy, as tragedy, or as an epic of cyclical return are all suggested but refused in favor of living with historical uncertainty. Only in her closing call to a feminist politics based on the lessons of history does she suggest—by acknowledging the hope that her readers will learn from history—that rhetorical uncertainty might itself be a heuristic device toward the construction of a feminist comedy.

Finally, I want to call attention to a last possibility opened up by the folklorist Henry Glassie in a recent number of the *Journal of American History*. Glassie proposes that the historical field "is the transitory one described by the Buddha, so complicated in its fluid intermixtures of cause, so multiplex in its overlappings of temporal rhythms, that origins dissolve into oblivion, connections ramify toward infinity, and only the predicament of the moment as understood by a flawed mortal can determine which pieces of the past are pertinent." History therefore cannot be forced into a single paradigm: strings of sequential change, even progressive change, occur but do not shape the whole. As a result, "every narrative, blatant in its incompleteness, must come upon the mind as an artifice, a willed confection, always questionable." The natural rhythm of history is the cyclical rhythm of the endless construction and reconstruction of authorizing stories, a rhythm that well suits the open-ended complexities of the historical field. A clear-eyed vision from any perspective suggests that "things get better. And they get worse."¹⁰¹

Glassie attempts to describe the shape of history that is congenial to his anthropological sensibilities and also implicit in the postmodern critique of historical knowledge. It is a shape that is shapeless, a shape but not a grand narrative. Yet it is a shape, precisely by its refusal of grand narrative, that potentially safeguards the integrity of every actor in the historical field and validates the limited point of view of every historical observer. Although Glassie's preferred historians range

⁹⁹ Judith R. Walkowitz, *City of Dreadful Delight: Narratives of Sexual Danger in Late-Victorian London* (Chicago, 1992).

¹⁰⁰ Walkowitz, *City of Dreadful Delight*, 244.

¹⁰¹ Henry Glassie, "The Practice and Purpose of History," *Journal of American History*, 81 (1994): 964, 966, 968.

from Edward Gibbon to Laurel Thatcher Ulrich, his favorite is Hugh Nolan, the clear-eyed reporter of better and worse, who tells his Irish neighbors the tales of his people's history. I assume that it is this ability of Glassie's vision to satisfy the populist principles of the *JAH*'s editor, David Thelen, that led him to feature the article.¹⁰² In Glassie's historical world, noble action does not require a grand narrative that promises a happy ending.

Postmodern theory does offer, then, some roads out of current historiographical dilemmas, but—of course—at a price. The regulative or heuristic adoption of grand narratives is in many ways the easiest to imagine and construct, allowing historians to explore, as they question, familiar stories. Indeed, the problem with this approach is that the stories can be too familiar, evoke too easily formalized doubts rather than deep, substantive questions. Something of this sort can be said against Rorty's ethnocentric bourgeois liberalism. Although Livingston questions aspects of the market-driven liberal comedy that he adopts as a heuristic principle, he relies too heavily on a strained interpretation of William James's pragmatism and accepts a good deal of unexamined baggage. For example, he seeks "to specify the generally American character or qualities of [modernist] intellectual change and innovation" and merely assumes that "the intellectual revolution here was broader, deeper, and more consequential than elsewhere."¹⁰³ American exceptionalism remains a powerful lure for any grand narrative constructed around American national history, and the one sure-fire cure, transnational history, is as yet little practiced.¹⁰⁴ Still, historians for whom grand narrative remains attractive yet elusive have everything to gain from a self-conscious reexamination of conceptual categories and rhetorical strategies.

The resistance to resolving uncertainty exemplified by Walkowitz invites more rigorous inquiry, but it also challenges the historian's capacity to deal with change over time. Part of the reason she can remain poised on the lip of a slide into progress, decline, or something in between is her focus on a very short period of time that can be viewed as a cross-section of history. Once the historian has to make change over time the focus of inquiry, it will be more difficult to render the direction uncertain and will probably be counterproductive to the inquiry itself.

Change over time would not be a problem, however, in Glassie's historical field, but that is because he has given up the possibility of progress or of any grand narrative. Whatever the shape of particular patterns of change over time, they would be provisional to the larger ebb and flow of history. Glassie's vision can satisfy the democratic particularism of recent social-cultural history; however, it demands in return the abandonment of the larger hope of progress. Indeed, even particular narratives are unstable, for the tone that suffuses the entire historical field is the choice of the viewer. Glassie invests it with a vibrant humanistic light. To him, a transitory Buddhist historical world is not what it is to Buddhists—a world of suffering that invites entrance into the permanent; rather, it is a rich

¹⁰² Those sympathies are clear in David Thelen, "The Practice of American History," *Journal of American History*, 81 (1994): 933–60.

¹⁰³ Livingston, *Pragmatism and the Political Economy*, 130.

¹⁰⁴ On transnational history, see Ian Tyrrell, "American Exceptionalism in an Age of International History," *AHR*, 96 (October 1991): 1031–55; and Higham, "Future of American History," 1303–04.

world of choices through which human beings attain their “humanity.”¹⁰⁵ Another postmodern theorist of history, Frank Ankersmit, sees the historical field only as a heap of leaves, already blown off the historiographical tree in this autumn of overproduction and loss of direction.¹⁰⁶ It is not clear how many American historians, nurtured on greater expectations, will want to take their democratic hopes onto such shifting terrain. Nonetheless, postmodern historical strategies offer a way to explore the provisional faiths, the uncertainties, and the personal visions of this doubtful moment.

WE HAVE COME A LONG WAY since Sloane inaugurated the *American Historical Review* with his assurance that he knew the grand narrative of history. Or perhaps a short way on the “weary search” he forecast if it were lost.

¹⁰⁵ Glassie, “Practice and Purpose of History,” 966.

¹⁰⁶ F. K. Ankersmit, “Historiography and Postmodernism,” *History and Theory*, 28 (1989): 149–50.

A Century of Writing Early American History: Then and Now Compared; Or How Henry Adams Got It Wrong

GORDON S. WOOD

JOHN FRANKLIN JAMESON, the first editor of the *AHR*, naturally wanted something from Henry Adams for the initial issue. Adams was, after all, “the foremost of our historical writers.”¹ But Jameson could hardly have been prepared for what he eventually got. Adams tried to put Jameson off. He was tired of history, he said. Since history had become for him “a sort of Chinese Play, without end and without lesson,” he had stopped teaching it, had not written a line of it for six years, and indeed had “forgotten what little history I ever knew.” All that he felt able to supply to Jameson in 1895 was a curious piece titled “Count Edward de Crillon.”² It was the last bit of orthodox history that Adams ever wrote.³

The essay corrected a minor error Adams had discovered in his *History of the First Administration of Madison*, which had been published in 1890–1891. It was, he dryly added, “one of these blunders, which is fortunately of so little consequence as to allow of attaching a story to it.” The story, which involved a French imposter in Washington in 1812, was indeed so insignificant that it suggested Adams was mocking the new historical professionalism expressed in the founding of the *AHR*. Adams opened his article with a brief disquisition on the problem of accuracy in historical writing. Errors, he said, creep in all through the process of history writing, and “the sum of such inevitable errors must be considerable.” Historians, he estimated, could count on only four out of five of their statements of fact to be exact. By this reckoning, said Adams, a history such as Macaulay’s, which had more than 150,000 assertions or assumptions of fact, probably contained at least 30,000 more or less inexact facts. The consequence for the writing of history was despairing: “Some historians are more, some less, inaccurate; but the best must always stand in terror of the blunders which no precaution and no anxiety for truth can save him from committing.” Not only did the participants in the historical process misunderstand themselves and their motives, but their “confusion of error” was aggravated by the personal mistakes of the historian. Each error builds upon another, and “tends to distort the whole subject,

¹ John Franklin Jameson to Waldo G. Leland, November 21, 1910, quoted in Morey Rothberg and Jacqueline Goggin, eds., *John Franklin Jameson and the Development of Humanistic Scholarship in America* (Athens, Ga., 1993–), 1: 340.

² Henry Adams to John Franklin Jameson, June 10, 1895, June 24, 1895, and November 17, 1896, in J. C. Levenson, et al., eds., *The Letters of Henry Adams* (Cambridge, Mass., 1988), 4: 286, 295–96, 440; Henry Adams, “Count Edward de Crillon,” *AHR*, 1 (1895–96): 51–69.

³ William H. Jordy, *Henry Adams: Scientific Historian* (New Haven, Conn., 1952), 129.

and to disturb the relations of every detail." "The result," wrote Adams, "becomes an inextricable mess." But, fortunately, not too much damage is done because few people read history anyway. Indeed, "conscious of the pitfalls that surround him, the writer of history can only wait in silent hope that no one will read him,—at least with too much attention."⁴

This ironic and jaded tone is totally out of accord with the rest of the first issue of the *AHR*, and Jameson's acceptance of what he called "a chip from [Adams's] workshop" is puzzling.⁵ No doubt after begging Adams for something, he was in no position to reject the great man's contribution. At any rate, its world-weary satirizing of historical accuracy and fact-finding starkly contrasted with the optimistic hopes and scientific aims of the fledgling historical profession in 1895. This *fin de siècle* piece could have been written today.

Nothing was more important to the historians who contributed to the *AHR* in the 1890s than accuracy and objectivity. The members of this founding generation aimed to make their discipline of history scientific. Of course, they were not such believers in science and objectivity that they ignored historical relativism. They argued explicitly, as Professor Herbert L. Osgood of Columbia University did, for "the principle of historical relativity," so that events and actions could be judged in accord with their historical contexts and not in accord with "latter-day ideals."⁶ They realized, too, as William M. Sloane declared in the opening essay of the first issue of the *AHR*, that "history will not stay written. Every age demands a history written from its own standpoint."⁷ But they did want to get their facts and footnotes straight. Frederick Jackson Turner almost did not turn in his carefully wrought article, "Western State-Making in the Revolutionary Era," to Jameson in time for the first volume, so conscientious was he in checking his evidence.⁸ These early professionals not only established the *AHR*, but they also created most of the conventions and rules by which the craft of history writing is still practiced.

IN ONE RESPECT and in one respect only does Adams's essay resemble the articles in the *AHR*: it falls within what is generally considered to be the period of early American history—American history up to 1815 or so. It is extraordinary to see the extent to which early American history dominates the early numbers of the *AHR*. For many of this first generation of professionals, it seemed that there was no other history but that of early America. In the first issue of the *AHR*, three out of the five articles dealt with early America, including that by Henry Adams. In fact, in the initial four issues of the first volume (1895–1896), over half the articles (twelve out of twenty-three) were located in early America. During the first five years of the *Review's* history (1895–1899), forty out of 108 articles were in the

⁴ Adams, "Count Edward de Crillon," 51–52.

⁵ John Franklin Jameson, "The American Historical Review, 1895–1920," *AHR*, 26 (1920–21): 9.

⁶ Herbert L. Osgood, "The Study of American Colonial History," *Annual Report of the American Historical Association for the Year 1898* (Washington, D.C., 1899), 71.

⁷ William M. Sloane, "History and Democracy," *AHR*, 1 (1895–96): 5. So, too, did Jameson declare that "each age has its own fashion in the writing of history." John Franklin Jameson, "Gaps in the Published Records of United States History," *AHR*, 11 (1905–06): 819.

⁸ Ray Allen Billington, *Frederick Jackson Turner: Historian, Scholar, Teacher* (New York, 1973), 164–65.

colonial, revolutionary, and early national periods of American history. Never in American history has early American history seemed so important and vital to the professional writing of history as it did a century ago.

Of course, a century ago, early American history was much closer to the contemporaries of 1895 than it is to us today, and there was much less modern American history to write about. Still, under any circumstances, it was an extraordinary dominance. The *Review*, after all, was supposed to take all of history, not just American history, as its domain. But in the four issues of the first volume of the *AHR*, only seven articles out of twenty-three were concerned with non-American history.

This attention paid to early American history was not new. Historians interested in the roots of the nation had always gone back to its colonial beginnings; indeed, nearly all of America's great historians have felt the need to go back to the colonial origins of the United States in order to find some basic points of reference for national self-consciousness. George Bancroft declared that he "dwelt at considerable length" on the seventeenth century in his great history of America "because it contains the germ of our institutions."⁹

Bancroft began his history of America with the Icelandic voyages and Columbus's discovery and had intended to bring it up to his own time, but in the end he carried his ten-volume history only through the peace with England in 1783. (Only later did he add the formation of the Constitution.) For Bancroft, the colonial period seemed to be the source of the whole of American history. It was the nation's youth. It flowed naturally into the national history that followed and was intimately connected with it. After all, "the maturity of the nation is but a continuation of its youth." Naturally, that youth did not know all that the mature nation knew. The early colonists did not know about religious freedom and democracy as people of the nineteenth century did, but the seeds of these developments were planted in the colonial period. The American people had gradually learned about them and had progressed. Bancroft's view of the colonial past was highly Whiggish; he saw the past as simply an anticipation of the present. In Volume 1 of his fifteenth edition, he titled the chapter describing the early English voyages to North America, including the Roanoke settlement in the 1580s, "England Takes Possession of the United States."¹⁰

Subsequent nineteenth-century writers of history remained preoccupied with the colonial and revolutionary periods as the seedbed of the United States. Not only did the amateur dabblers in antiquities centered in the state local historical and genealogical societies focus on the colonial sources of America, but Herbert Baxter Adams, often considered, perhaps mistakenly, to be the first of the American professional historians, devoted most of his energies in the 1870s and 1880s to promoting the study of local institutions in the colonial period, including

⁹ George Bancroft, *History of the United States from the Discovery of the American Continent*, 10 vols. (1837–66; Boston, 1853), 1: vii.

¹⁰ Bancroft, *History of the United States*, 1: vii, 74. On Bancroft's Whiggish and prehistoricist thinking, see Dorothy Ross, "Historical Consciousness in Nineteenth-Century America," *AHR*, 89 (October 1984): 915–19.

the New England town.¹¹ It was, in fact, against this antiquarian history writing of the amateurs, and the local scholarship of his teacher H. B. Adams, that Jameson, the first Ph.D. under Adams at Johns Hopkins, directed his professional and scientific scorn. In his retrospective account of the origins of the *AHR*, Jameson, its indefatigable editor, condescendingly described the *Review's* predecessors, the *Historical Magazine* and the *Magazine of American History*, as useful journals but ones that “belonged to and represented a period when the little military engagements of the Revolutionary War, the biographies of its heroes and of the ‘Fathers’ in general, the minutiae of voyages and discoveries, endlessly disputable, and the local and antiquarian details of the colonial period, were regarded as the main matters of American history.”¹²

Like Jameson, Charles McLean Andrews, the best scholar to come out of Adams’s Johns Hopkins seminar, argued that nineteenth-century historians had been much “too provincial” in their attitude toward early American history. His doctoral dissertation on three colonial Connecticut towns shattered the idea that local institutions were the principal sources of American development. By the 1890s, he was openly pleading for more cosmopolitan perspectives on the colonial period. He criticized his predecessors for concentrating on each of the thirteen particular colonies at the expense of the whole, for ignoring the other British colonies in the Western Hemisphere, and for neglecting the period from 1690 to 1750, since it lacked “striking incidents and critical movements.” Bancroft, wrote Andrews in 1898, devoted less than 200 pages of his ten volumes to this period, and he spent most of the 200 pages on the wars. Although Richard Hildreth, in his six-volume *History of the United States*, gave more pages to the period, he showed no appreciation of the events he chronicled. J. A. Doyle, “whose three volumes on the colonies are unquestionably the best that we have,” seemed to have abruptly ended his narrative in 1700, and “thus the historian who might have taken the proper point of view has deserted the field.”¹³

In the same year, Herbert L. Osgood likewise complained of the trivial state of knowledge about colonial America. He admitted that his predecessors had had “no lack of zeal or labor” in writing about early American history and that consequently “a vast literature upon the colonial period exists.” Nineteenth-century historical and genealogical societies had devoted most of their energies to early American history. State and local historians had in fact written about little else than the colonial period. Still, when all was said and done, said Osgood, “the colonial period of American history is not well understood.” Too much of nineteenth-century colonial history remained “petty” and “antiquated.” Most of the state and local historians had no ability to distinguish between “the really important and the insignificant,” and thus most of the histories they wrote were

¹¹ John Higham, “Herbert Baxter Adams and the Study of Local History,” *AHR*, 89 (December 1984): 1225–39.

¹² Jameson, “American Historical Review, 1895–1920,” 1.

¹³ Charles M. Andrews, “American Colonial History, 1690–1750,” *Annual Report of the American Historical Association for the Year 1898*, 49, 60, 49–50; Higham, “Herbert Baxter Adams and Local History,” 1236. Eventually, after an eighteen-year lapse, Doyle did add two more volumes to his *History of the English Colonies in America* (New York, 1889). These were titled *The Middle Colonies* (New York, 1907) and *The Colonies under the House of Hanover* (New York, 1907).

little more than annals. These historians had no principle of organization and had tended to treat each colony in isolation from all the others. They generally had ignored the colonies outside of New England and in fact had allowed "New England ideas" to dominate whenever they did discuss the other colonies. He urged historians, as Andrews had done, to look at early American history "not only from the colonial but from the British standpoint; full justice must be done to both sides."¹⁴

But there was a new day coming. By 1895, Jameson and his colleagues believed that the study of history in the United States, including the study of early American history, was passing into a more advanced stage of development. They marveled at the "immense change which has come over the character of history," expressed by the publication of the *AHR*, and they were excited by the emerging professionalism of the discipline of history.¹⁵ They knew that much of historical study in the past had been erroneous, yet they were confident of the future. Their outlook was optimistic, cosmopolitan, and judicial. These new professional historians called for the broadening of outlook, greater disinterestedness in evaluation, and a widening of subject matter. Osgood in a moment of euphoria even suggested that historians ought to investigate the colonial period "with a view to the ascertainment of its position in the general history of the world."¹⁶ Some, like William M. Sloane of Princeton, believed that the new professional historians had already "revolutionized" historical scholarship "in that we no longer study nations, but epochs."¹⁷

Although these calls to look beyond the nation, in particular the efforts of imperial-minded historians to make colonial history mainly a branch of British history, would eventually have profound effects on early American history writing, most historians in the 1890s continued to assume that explaining the roots of the United States and its institutions was their preeminent task. So Carl Becker's initial essay for the *AHR* was on the origins of the modern nominating convention or, as he put it, a "search for the beginnings of the democratic method," which meant going back to "the period which marks the rise of democracy itself—that is, the eighteenth century." So, too, Turner's first essay in the *AHR* was an attempt to describe the "process of Americanization" that took place in the late eighteenth-century West. Even those imperial historians such as Andrews and Osgood who wished to view early America from the vantage point of London conceded that the colonial period was first of all "a period of origins."¹⁸ This

¹⁴ Osgood, "Study of American Colonial History," 63–73.

¹⁵ Sloane, "History and Democracy," 5.

¹⁶ Osgood, "Study of American Colonial History," 65.

¹⁷ Sloane, "History and Democracy," 4.

¹⁸ Carl Becker, "Nominations in Colonial New York," *AHR*, 6 (1900–01): 270, 260; Frederick Jackson Turner, "Western State-Making in the Revolutionary Era," *AHR*, 1 (1895–96): 70; Osgood, "Study of American Colonial History," 66. Although Jameson was sympathetic to a broad conception of history, he, like most of his colleagues, had no doubt "that for the last four hundred years the actual form in which human life has mainly run its course has been that of the nation." Since "the whole course of American history thus far has lain in the era of nations, during which the most potent and visible unity of human affairs was the political," it was natural for historians to focus on "the constitutional and political history of the United States and of the colonies out of which they grew." Jameson, "Gaps in the Published Records," 819–20.

traditional preoccupation with the origins of the nation thus underlay the dominance of early American history in the beginning years of the *AHR*.

Despite its professions of cosmopolitanism, this founding generation of historians often seemed as parochial in outlook and as bound by the culture in which they lived as their predecessors were. Although Jameson warned his colleagues that they “must not forget that our origins, even our constitutional and political origins, are not all English,” most of them were unabashedly and explicitly Anglo-centric.¹⁹ The rapprochement between the United States and Great Britain that took place at the end of the nineteenth century obviously influenced the imperial perspective of Andrews and Osgood and the new sympathetic appreciation of the loyalists that was revealed in Moses Coit Tyler’s contribution to the first issue of the *AHR*. Albert Bushnell Hart of Harvard urged that studies of the American Revolution be written in order mainly to “discover the real causes of that great division in the English race.”²⁰ And Tyler called the breakup of the British Empire “a race-crisis.”²¹ The report of the professional historians’ Committee of Seven suggested in 1899 that history teaching in the high schools be structured in a four-stage process: ancient history the first year, European history the second, British history the third, and American history at the end. This structure, the report stated, “reveals a natural sequence of events” and covers “the whole field of history.”²²

IT IS NO DOUBT EASY to mock, as Henry Adams did, the dreams and the naïve enthusiasm for accuracy and objectivity of this first generation of professional historians. But we too blithely forget the hodgepodge of myths and fraudulent claims about the past that they had to contend with and try to rectify. The nineteenth century, as A. B. Hart pointed out, had littered the colonial past with myths such as the stories of the Pilgrim Fathers and Plymouth Rock and Parson Weems’ concocted tales about the cherry tree and young George Washington. All that mythical litter had to be cleaned up, and all those invented and forged documents, such as the *Letters of Montcalm* and the *Travels of Jonathan Carver*, had to be exposed and eliminated from the country’s historical consciousness. Only the early profession’s scrupulousness about accuracy led to the revealing of William Gordon’s *History of the Revolution* as largely borrowed from the Annual Register, or to the exposing of the fabricated eighteenth-century North Carolina newspaper that had sought to legitimate the Mecklenburg Resolutions of May 20, 1775, or to the uncovering of the many errors and imagined sources in a new biography of John Paul Jones.²³ This founding generation took seriously their

¹⁹ Jameson, “Gaps in the Published Records,” 822.

²⁰ Albert B. Hart, “The Historical Opportunity in America,” *AHR*, 4 (1898–99): 13.

²¹ Moses Coit Tyler, “The Party of the Loyalists in the American Revolution,” *AHR*, 1 (1895–96): 25.

²² Review of Andrew C. McLaughlin, *et al.*, *The Study of History in Schools: Report to the American Historical Association by the Committee of Seven* (New York, 1899), in *AHR*, 5 (1899–1900): 320–23.

²³ A. B. Hart, “Imagination in History,” *AHR*, 15 (1910): 230–32, 242; Edward G. Bourne, “The Travels of Jonathan Carver,” *AHR*, 11 (1905–06): 287–302.

responsibility to police the past and to correct these frauds and mistakes; it was part of what they meant by being scientific.

Although these new professional historians talked endlessly about history becoming scientific, they did not expect history to discover laws of human behavior and become like sociology—God forbid! There was nothing they feared or disliked more than sociology. By calling history scientific, they merely meant that it should be carried out in accord with the scientific method as understood by Francis Bacon. That is, they intended to be as strictly empirical as they could. They wanted, wrote Hart, who was one of the most Baconian of the new professional historians, a scientific history “which shall remorselessly examine the sources and separate the wheat from the chaff; which shall critically balance evidence; which shall dispassionately and moderately set forth results.”²⁴

Scientific history was judicious history. Previous historians had often been biased and unfair. Bancroft, for example, had simply dismissed the English navigation system for the colonies as “a badge of servitude,” instead of treating it as “a natural and necessary phase in the development of colonization.”²⁵ In contrast to their predecessors, these early professionals saw themselves as impartial judges rendering disinterested judgments on the past. Indeed, as Moses Coit Tyler remarked, “a disinterested attitude” was identical to “an historical one.” They believed, as Tyler put it, “that the truth is to be found only by him who searches for it with an unbiassed mind.” Historians, maintained Osgood, had to have the “breadth of information and the catholicity of spirit sufficient to do justice to both parties” in the conflict between the colonies and Great Britain. To do justice, facts, facts, and more facts were needed; only through immersion in the facts, or evidence, would historians, or “scientific investigators,” as Tyler called them, be prepared to make impartial judgments about the past.²⁶

By facts, most historians of the period meant documents. Articles and reviews were ephemeral, said Jameson, but documents were permanent: “original materials usually retain their value unimpaired.”²⁷ History for Jameson and his generation was mainly a question of collecting all the documents, by which they meant not impersonal records of numerous births and deaths or lists of wheat prices but consciously created, motive-revealing written documents—such as letters, journals, reports, and pamphlets. “When all the documents are known, and have gone through the operations which fit them for use, the work of critical scholarship will be finished.” So wrote the French historians Charles Victor Langlois and Charles Seignobos in the English translation of the most popular and influential manual of historical method used by American historians of the day.²⁸

The early issues of the *AHR* tried to meet this need for documents. The *Review* devoted pages to lists of bibliographical collections, extending from the holdings of the American Antiquarian Society, the greatest repository of early Americana in

²⁴ Hart, “Imagination in History,” 232.

²⁵ Osgood, “Study of American Colonial History,” 70–71.

²⁶ Tyler, “Party of the Loyalists,” 26, 27; Osgood, “Study of American Colonial History,” 72.

²⁷ Jameson, “American Historical Review, 1895–1920,” 10.

²⁸ Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (Cambridge, 1988), 39.

the country, to the materials in the Public Record Office relating to the history of the British colony of West Florida. The hodgepodge of documents included in every issue implied that anything and everything that was consciously written might prove helpful to the science of history. Like the articles, most of the documents concentrated on the colonial and revolutionary periods. They ranged from a letter to George Washington to a list of printed commissions and instructions to colonial governors, from accounts of emigration from Yorkshire to West Jersey in 1677 to a letter of Jefferson on political parties, from selections of Philip Fithian's eighteenth-century journal kept while he was a tutor in a rich Virginia planter's household to documents concerning the relationship between the Council for New England and the Merchant Venturers of Bristol, England, in 1621–1623.

A. B. Hart nicely expressed the determination and optimism of this early generation of historians in his essay "Historical Opportunity in America," published in Volume 4 of the *Review*. Hart had a deep-rooted concern for preservation, both of documents and of historical sites, and a passionate interest in the teaching of history in the schools and in the larger society that appear as vital and as applicable today as they did a hundred years ago. He called for the identifying and marking of important historical buildings and sites and for doing whatever was necessary to inform the public about the past. Indeed, he was eager to exploit all sorts of up-to-date methods of popular historical instruction, including the use of lantern slides. Although Hart was suspicious of amateurs dabbling in history (the "Sons and Daughters of Historical Periods . . . do not often promote exact historical work"), he was generous in his praise of "the much abused" antiquarians who did so much to collect and preserve the records of the past.²⁹

But, as important as buildings and sites were for the people's understanding of the past, professional historians required official records of events. And, for Hart, most of the records and much of the opportunity for historical work were in the colonial and revolutionary periods. There, the need for documents of all sorts was greatest. Although the thirteen colonial legislatures and many of the New England towns and southern counties had published selections from their records, "we have nothing approaching complete printed records of a single colony." Local records, he wrote, had been "notoriously neglected," and the archives of the states were "in a deplorable condition." To aid the historical "seeker after truth," Hart urged the preservation and calendaring of records at all levels.³⁰ But certain kinds of records, he believed, ought to be published, particularly those having to do with the revolution and the formative period of the national government. He called for a new edition of the journals of the Continental and Confederation Congresses, to be followed by the publication of selections from the manuscript reports of committees and other congressional papers of revolutionary times. Next, "new national editions" of the works of the great American statesmen of the founding era were needed. It was not suitable, he asserted, that the papers of Washington and Jefferson and others, though "the property of the government,

²⁹ Hart, "Historical Opportunity in America," 5.

³⁰ Hart, "Historical Opportunity in America," 7.

should contain material known to the public only through very expensive limited editions of private publication." Of course, historians had to be grateful for documents in any form, but "we should be more grateful if the government would itself issue in generous and scholarly editions the works of the great statesmen."³¹

Even more important to the developing science of history, argued Hart, was the monograph. As late as the 1870s, most historical work had appeared either in elaborate multivolume works or in periodical articles. Although some systematic monographs had begun to appear in the Johns Hopkins Studies, most of these were much too local for this new breed of historians. Hart had a broader sense of what was needed, and he listed some of the topics in early America that required monographic investigation: a constitutional history of the colonial period, a constitutional history of the revolution, a history of slavery, a study of the moral and business standards of the colonists, a history of land and land tenure in early America, and a history of religion in early America.

Hart's list and the early issues of the *AHR* reveal just how rudimentary was the knowledge of early America a hundred years ago. Almost anything, whether it was an article on the proprietary province as a form of government or a piece on the British taxation of tea from 1767 to 1773, was an addition to historical knowledge. One of the big arguments in the early volumes of the *AHR* even involved the authorship of the *Federalist Papers*—an issue resolved only within the past several decades.³²

THIS FIRST GENERATION of historians had no clear picture of what would come of their collective production of monographs. "I struggle on," Jameson told Henry Adams in 1910, "making bricks without much idea of how the architects will use them, but believing that the best architect that ever was cannot get along without bricks, and therefore trying to make good ones."³³ Despite all our modern refinement and theoretical sophistication, this assumption that historical understanding will come from the accumulation of many historical monographs still undergirds most of what present-day professional historians do. All the technical monographs that pour from the presses today in bewildering numbers—books, articles, newsletters, research reports, working papers by the thousands—are just so many bricks that we hope somehow can be fitted together by an architect or architects. We still cling to the belief expressed by Hart that "every small monograph is a contribution to the materials at the service of the whole body of learned men."³⁴

No doubt in our time, the production of monographs has become overwhelm-

³¹ Hart, "Historical Opportunity in America," 6–8. On the importance of publishing early American documents, see also Jameson, "Gaps in the Published Records," 817–31.

³² Edward G. Bourne, "The Authorship of *The Federalist*," *AHR*, 2 (1896–97): 443–60; Paul L. Ford, "The Authorship of *The Federalist*," with a reply by Bourne, *ibid.*, 675–87. For the modern resolution of authorship, see Douglass Adair, "The Authorship of the Disputed Federalist Papers," *William and Mary Quarterly*, 3d ser., 1 (1944): 97–122, 235–64; Frederick Mosteller and David L. Wallace, *Inference and Disputed Authorship: The Federalist* (Reading, Mass., 1964); and Jacob E. Cooke, ed., *The Federalist* (Middletown, Conn., 1961), xi–xxx.

³³ John Higham, *History: Professional Scholarship in America* (1973; Baltimore, Md., 1989), 24–25.

³⁴ Hart, "Imagination in History," 233.

ing, and we seem not to know where any one of the multitudes of bricks is supposed to belong or what the larger structures are supposed to look like.³⁵ That first generation of historians also felt “overwhelmed with reading matter,” but the feeling did not stop them from wanting more articles and books.³⁶ Even though having too much history presents problems and challenges, it is scarcely a reason for the hand-wringing jeremiads about the decline of the discipline of history that we so often hear today. We are enjoying the bountiful fruits of the seeds planted by our professional predecessors of a century ago.

It is hard today to see early American history in any sort of real crisis. What we have instead is an embarrassment of riches. While writing about all aspects of American history has been rapidly expanding in all directions during the past several decades, writing about the colonial period has virtually exploded. Early American history, according to Bernard Bailyn, the historian who over the past thirty years has dominated the period as much as any single person could, is experiencing a “creative ferment of scholarship” that is unparalleled in American history writing. Indeed, this explosion of scholarship has produced “a wealth of research and writing concentrated on a relatively short period of time that is perhaps unique in western historiography.”³⁷

Although the numbers of monographs on early American history may now seem overwhelming, collectively they are the source of our remarkable advance in historical knowledge. It is impossible to deny that real progress in our understanding of early American history has taken place since 1895. Not only do we know more new things about early America than ever before, but we know more about old things as well. Subjects that in the 1890s had a single article written about them, such as the British convicts shipped to America or the English and Dutch in New Netherlands or the Loyalists in the American Revolution, now command entire books.³⁸ Most of the monographs that Hart called for—on constitutionalism in the revolution, on slavery, on economics, on religion—have been written two and three times over. Not only do we now know who wrote the several *Federalist Papers*, but we also have many modern studies of imperial policy and of the colonies outside New England; and, thanks to present-day historians

³⁵ David Thelen, “The Practice of American History,” *Journal of American History*, 81 (1994): 937.

³⁶ Hart, “Historical Opportunity in America,” 3.

³⁷ Bernard Bailyn, *The Peopling of British North America: An Introduction* (New York, 1986), 6. For appreciations of Bailyn’s contributions to early American scholarship, see Michael Kammen and Stanley N. Katz, “Bernard Bailyn, Historian and Teacher: An Appreciation,” Gordon S. Wood, “The Creative Imagination of Bernard Bailyn,” and Jack N. Rakove, “‘How Else Could It End?’: Bernard Bailyn and the Problem of Authority in Early America,” in James A. Henretta, *et al.*, eds., *The Transformation of Early American History: Society, Authority, and Ideology* (New York, 1991), 3–69.

³⁸ The pioneering article by James D. Butler, “British Convicts Shipped to American Colonies,” *AHR*, 2 (1896–97): 12–33, was supplemented, first, by Abbot E. Smith, *Colonists in Bondage: White Servitude and Convict Labor in America, 1607–1776* (Chapel Hill, N.C., 1947); then by A. G. L. Shaw, *Convicts and the Colonies: A Study of Penal Transportation from Great Britain and Ireland to Australia . . .* (London, 1966); and, most recently and fully, by A. Roger Ekirch, *Bound for America: The Transportation of British Convicts to the Colonies, 1718–1775* (Oxford, 1987). Albert E. McKinley, “The English and Dutch Towns of New Netherland,” *AHR*, 6 (1900–01): 1–18, was an early effort at multicultural history, and it has been followed up in recent years by a number of books. Likewise, in the past twenty years, professional scholarship on the Loyalists, more or less introduced by Moses Coit Tyler in the first issue of the *AHR*, has greatly expanded.

such as Jack P. Greene and members of the Chesapeake School, "New England ideas" no longer dominate colonial historiography.

Our knowledge of America's colonial past is clearly richer, deeper, and fuller than ever before. Much of what the historians of the 1890s could only dream of has steadily been realized. Mammoth publication programs that are now nearly a half-century old and that will extend well into the next century are gradually putting into magnificent letter-press editions the papers of the founding fathers such as Thomas Jefferson and George Washington and the writings of other important figures of early America such as William Penn and Jonathan Edwards. We no longer have to rely on a few pages of Fithian's journal in the *AHR* as the historians of 1890s had to. Not only do we now have a full published edition of Fithian's journal, but scarcely a month goes by that another diary of some obscure early American is not discovered and put into print. We have access to more printed documents about early America than we can ever hope to manage, and microfilm, computers, CD-ROM, and other modern techniques are making available many others; in fact, the new techniques are capable of actually creating documentary evidence that the historians of a century ago could not have imagined existed.

Despite the popularity of books such as Peter Novick's *That Noble Dream* and the spread of epistemological skepticism to parts of the historical profession, most historians of early America seem to be going about their craft today in much the same manner as their predecessors of a century ago. Although there are clearly enormous differences between the history written a century ago and that written today, those differences have almost nothing to do with the so-called "objectivity question" that bothered Henry Adams and has recently worried some of the profession.

The most obvious of the differences that do exist between then and now is the present-day emphasis on social and cultural history and the relative neglect of political and institutional history. Although this new social and cultural history is usually associated with developments of the past two or three decades, its roots actually go back at least a half-century, to the growing willingness of mid-twentieth-century historians to exploit the concepts and theories of social science.

The results of this exploitation for early American history in the past half-century have been extraordinary. New areas have been opened up for research and study, and lost worlds of the past have been recovered. Although present historians of early America still write about political institutions and constitutionalism, they now write about everything else as well. In fact, there is scarcely an aspect of human behavior in early America that historians today do not write about—from divorce to dying, from the consumption of goods to child rearing. Historians such as Philip Greven and Jan Lewis have delved into the most private, subjective, and least accessible aspects of the past in early America, including marriage, sexual relations, and child abuse. Social science, especially anthropology and ethnography, has enabled historians such as Rhys Isaac and T. H. Breen to reconstruct from riots, rituals, and other kinds of popular nonverbal behavior in the past the beliefs and attitudes of the masses of ordinary men and women who left no written record. At the same time, historians such as John Demos and David

Hall have used social science to write sympathetically of magic, witchcraft, and many other subjects that used to be thought of as the irrationalities and superstitions of the pre-modern past.

The historians of 1895 were well aware that there was more to history than politics and constitutionalism; they knew there were all sorts of social and cultural subjects that had histories. William M. Sloane in his opening essay in the *AHR* declared that “mere political history . . . will no longer suffice for a public hungering after information.” People want to know as well about “the social, industrial, commercial, aesthetic, religious, and moral conditions of the common man.”³⁹ Jameson and the other editors of the *AHR* wanted the books they reviewed to “represent all fields and varieties of history—not political history alone, but also ecclesiastical, legal, military, naval, economic, social, and cultural history, and the general history of science and of literature.”⁴⁰ The *AHR* in 1899 even published a brief excerpt from Edward Eggleston’s manuscript that would become *Transit of Civilization*, describing some curious medical notions of the seventeenth century, although few historians of the day knew what to make of Eggleston’s odd collection of information.⁴¹ Both Jameson and Andrews called for a history of religion as the best means of getting at the thoughts and sentiments of ordinary people in the American past. Should we not treat the Great Awakening, asked Jameson, “as the most important and significant event” of eighteenth-century colonial America?⁴² In 1895, just as the *AHR* was being launched, Jameson gave a series of lectures at Barnard College on the economic and social aspects of the American Revolution, including landholding, business and industry, religion, and slavery—lectures that were published only a generation later as *The American Revolution as a Social Movement*.⁴³ That first generation of professionals was never as narrow as it has often been portrayed.

Nevertheless, the predominant view of the day was undoubtedly that of the imperial historian, Herbert L. Osgood. As important as the colonial land and trade systems, the religious beliefs, and the social customs of the colonists were, Osgood pointed out, these “social elements” took on meaning and became “in the large sense operative” only when they were organized through legal and political institutions.⁴⁴ To his dying day, Charles McLean Andrews could not figure out how to contain those social facts that lay outside the political institutions he had portrayed so brilliantly in his four volumes of *The Colonial Period of American History*. Although he hoped to write a fifth volume on eighteenth-century life that would be “an *omnium gatherum* of everything not political, institutional, or military,” he despaired of organizing it. Either the book would be “a sort of chaos of habits and customs, ways of living, dressing, eating, and the performance of the duties of existence,” or it would be a kind of “social

³⁹ Sloane, “History and Democracy,” 6.

⁴⁰ Jameson, “American Historical Review, 1895–1920,” 11.

⁴¹ Edward Eggleston, “Some Curious Colonial Remedies,” *AHR*, 5 (1899–1900): 199–206.

⁴² John Franklin Jameson, “American Acta Sanctorum,” *AHR*, 13 (1907): 299–300.

⁴³ John Franklin Jameson, *The American Revolution as a Social Movement* (Princeton, N.J., 1926).

⁴⁴ Osgood, “Study of American Colonial History,” 68–69.

science" that would be static, rigid, and abstract, lacking the vitality and movement of history.⁴⁵

SOCIAL SCIENCE or sociology was the great bugaboo of the founding generation of historians. Whenever some semblance of social history was advocated, most of them thought they were being led down the garden path to sociology. They believed that sociology, with its penchant for generalizations and overriding schemes, was simply "the ghost of our ancient enemy, the philosophy of history." Sociology was formulaic; it tortured the facts to fit hypotheses; and it drained all individuality, all particularity from life. No matter that sociologists accused historians of not being scientific, of spending "all their time in indexing dreary, profitless details about inconsequential folk, in developing their technical skill for the discovery of insignificant objects, in learning so much about how to investigate that they have forgotten what is worth investigation." The historians were proud of their absorption in details, in facts, in individuality. "If history is not a science of society, it is more; it is society, it is travel, acquaintance, experience, life."⁴⁶

Most historians of 1895, even if they had wished to write social and cultural history, were incapable of conceiving of it in the way present historians do. Their judicial-like preoccupation with the actions of individuals and their intense desire to distinguish their discipline from sociology prevented their thinking in structural terms about the past. They had little concern for those developments that took place, so to speak, over the heads of the historical participants—demographic movements, economic series, family cycles, and other large-scale aggregate patterns that were unknown to contemporaries but that have been historically recovered by the modern statistical and quantitative techniques of social science. Since few of these developments were recorded as events in the usual written documents of the past, most late nineteenth-century historians ignored them. With their judge-like perspective on the past, they tended instead to concentrate on those documents that revealed the intentions and motives of individuals. "The theme of history," said Professor George L. Burr of Cornell in 1903, "has indeed been . . . the lives and deeds of individuals—individual men, individual peoples, individual states, individual civilizations; its method has been, not biologic, but biographic; its prime aim, however obscured now and then by

⁴⁵ Charles M. Andrews, "On the Writing of Colonial History," *William and Mary Quarterly*, 3d ser., 1 (1944): 31–33.

⁴⁶ "The Meeting of the American Historical Association at New Orleans," *AHR*, 9 (1903–04): 449–50. Some historians continued to equate social history with sociology well into the second half of the twentieth century. When the *Journal of American History* in its March 1965 issue chose not to review Stephan Thernstrom's *Poverty and Progress: Social Mobility in a Nineteenth Century City* (Cambridge, Mass., 1964) but merely to list it in the *Journal's* "Book Notes," along with such throwaways as *The Secret Loves of the Founding Fathers*, I wrote the editor to express my astonishment. He replied that he had originally decided not to review Thernstrom's book, which was one of the pioneering studies of the new social history of the 1960s, because he and his colleagues considered it to be sociology and not "squarely in the field of American history." Oscar O. Winther, editor, *Journal of American History*, letter to the author, April 28, 1965.

the prepossessions, theologic or sociologic, of the historian, has always been, in the simple phrase of Ranke, to learn and to tell *wie es eigentlich gewesen ist*.”⁴⁷

We today can readily see that the historians of 1895 lacked what contemporary sociologists called “the social viewpoint,” that is, a conception of society itself as the organizing theme of their history.⁴⁸ Without a conception of society as a whole, they tended to see social history, in G. M. Trevelyan’s famous phrase, simply as “the history of a people with the politics left out.” In other words, social history for them became little more than aimless descriptions of the daily lives of people in the past, their conditions of work and leisure, the circumstances of their families and households, the forms of their religious and artistic thought, their customs and local folkways—interesting stuff, no doubt, but essentially antiquarian and trivial.⁴⁹ As sociologists such as Lester Ward charged, social history of this sort was just “a train of facts”; it was necessarily peripheral, superficial, and without any coherence.⁵⁰

All this changed with twentieth-century historians’ gradual acceptance of the conceptions and notions of social science. Once historians adopted society as a category of analysis and began to conceive of social history as the history of society, a radical transformation of their discipline became possible. Much of the social history of the past several decades rests on this conception of society as the main subject of historical work. And the American colonial period became an especially receptive place for the new social history; indeed, no period of American history has benefited more from the rise of modern social and cultural history.

Once society was thought of as something constructed, formed, shaped, and developed over time as the unintended consequences of many purposes and actions, then it was just a short step to thinking of culture in the same way, as *mentalités*, as something created and fabricated by many participants and susceptible to change. In both cases, the colonial period offered a wide-open field for study. It, unlike other periods of American history, was a time when society and culture were created virtually *de novo*, and thus it offered unusual opportunities for reconstructing the history of society and culture as they developed and changed. Benefiting from the techniques of the *Annales* school and Peter Laslett’s Cambridge Group, this new understanding of social and cultural history has made possible a variety of remarkable historical works in early American history over the past few decades.

Perhaps most impressive has been the historical recovery of the lost society of the seventeenth-century Chesapeake. The collective efforts of a number of historians, much of it quantitative, has gradually revealed to us a past colonial society in early Virginia and Maryland that we scarcely knew existed—a society in which life was nasty, brutal, and short, where diseases ran rampant, Indian conflict was constant, and parentless children and multiple marriages were the norm. By emphasizing in their scholarship the demographic and other structural processes that transcended the conscious intentions of the historical participants, various

⁴⁷ “Meeting of the American Historical Association at New Orleans,” 450.

⁴⁸ “Meeting of the American Historical Association at New Orleans,” 450.

⁴⁹ Harold Perkin, “What Is Social History?” (1953), in *The Structured Crowd: Essays in English Social History* (Brighton, Sussex, 1981), 1, 4–5, quotation on p. 1.

⁵⁰ “Meeting of the American Historical Association at New Orleans,” 450.

Chesapeake scholars have approached the society of the past in a manner that was inconceivable to most historians of 1895, preoccupied as they were with traditional written documents and individual motives. Yet, in their imaginative creation and accumulative assembling of brick-like articles and monographs into a collective configuration—an entire Chesapeake social world—these new social historians clearly fulfilled the highest hopes of the founding generation for the way in which the discipline of history would work to enlarge and deepen our understanding of the past. This collective reconstruction of seventeenth-century Virginia and Maryland society is surely one of the great achievements of modern historiography; and it is one of which Jameson and other historians of 1895 would have been proud.⁵¹

No doubt, the colonial period has been naturally receptive to social history because it had especially conspicuous examples of societies forming and developing. But this is not the only reason for its receptivity to the new social and cultural history. The colonial period is also the best arena in America's past for studying history in the *longue durée*. The seventeenth and eighteenth centuries have few headline political events: there are no presidents, no congresses, no supreme court decisions, no national elections to get in the way of long-term analyses of society. Colonial historians therefore have been freer than national historians to concentrate on social and cultural developments that go on longer than a few years or even a few decades. Where national historians tend to concentrate on periods of only several decades in length, early American historians usually regard themselves as historians of the entire span of colonial history. Only in the colonial period therefore could historians of America have the sweep of a century or more, uninterrupted by political events, in which to lay out long-term social and cultural developments. It is not coincidental that the modern study of American demographic and family history began first in the colonial period or that one of the earliest studies of American attitudes toward death concentrated on the colonial period.⁵² Historians cannot trace such enduring social and cultural subjects over only a decade or two; they need long stretches of time, which the colonial period offers.

⁵¹ The recent literature on the Chesapeake is voluminous. Some of the more notable works include Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975); Aubrey C. Land, Lois Green Carr, and Edward C. Papenfuse, eds., *Law, Society, and Politics in Early Maryland* (Baltimore, Md., 1977); Thad W. Tate and David L. Ammerman, eds., *The Chesapeake in the Seventeenth Century: Essays on Anglo-American Society* (Chapel Hill, N.C., 1979); Paul G. Clemens, *The Atlantic Economy and Colonial Maryland's Eastern Shore: From Tobacco to Grain* (Ithaca, N.Y., 1980); Gloria L. Main, *Tobacco Colony: Life in Early Maryland, 1650–1720* (Princeton, N.J., 1982); Darrett B. Rutman and Anita H. Rutman, *A Place in Time: Middlesex County, Virginia, 1650–1750* (New York, 1984); Allan Kulikoff, *Tobacco and Slaves: The Development of Southern Cultures in the Chesapeake, 1680–1800* (Chapel Hill, 1986); Lois Green Carr, Philip D. Morgan, and Jean B. Russo, eds., *Colonial Chesapeake Society* (Chapel Hill, 1988); and most recently, James Horn, *Adapting to a New World: English Society in the Seventeenth-Century Chesapeake* (Chapel Hill, 1994). The Chesapeake School also has produced many journal articles, too numerous to cite, but especially important are those by Lois Green Carr, Russell R. Menard, and Lorena Walsh.

⁵² Philip Greven, "Historical Demography and Colonial America," *William and Mary Quarterly*, 3d ser., 24 (1967): 438–54; David E. Stannard, *The Puritan Way of Death: A Study in Religion, Culture, and Social Change* (New York, 1977).

EVEN MORE IMPORTANT for the recent development of early American history have been the changes that have taken place in the society and culture of the United States over the past several decades. Just as the rapprochement between the United States and Great Britain in the 1890s helped to bring about a new imperial conception of the colonial period, so, too, are changing circumstances in America today affecting our approaches to the colonial past. The new emphasis on diversity and the new racial, ethnic, and gender consciousness have diluted and blurred a unified sense of American identity and have led to less and less emphasis on the nation as a whole in historical research and writing.⁵³ With the weakening or loss of a belief in the reality of any overall national identity, many historians in the past several decades have ceased looking to the colonial period for the origins or roots of the United States. They have, as Joyce Appleby remarks, found it “easy to abandon the idea that what was truly important about the colonies was their contribution to American nationhood.”⁵⁴ The consequence has been a number of shifts in perspective, leading to fundamental changes in the way early American history is conceived and written.

Some historians of early America have enlarged their perspective—no longer focusing exclusively on the territory that became the United States but, as Bernard Bailyn has put it, seeing early modern worlds in motion from a satellite hovering somewhere over the Atlantic.⁵⁵ A century ago, Herbert L. Osgood hoped eventually to see an American colonial history that “will be taken out of its isolation and will appear as a natural outgrowth of the history of Europe.”⁵⁶ We are well on the way not merely to fulfilling Osgood’s hope but to surpassing it. The colonies are now seen as an outgrowth not just of Europe but of Africa as well; they have become parts of a greater pan-Atlantic world. We are witnessing a vast broadening of the boundaries of what constitutes early American history. Indeed, the historical horizons on our colonial past have expanded to the point where they now seem limitless. For many historians, early American history is no longer what it was for Bancroft and most historians of the 1890s, a means of understanding the origins of the United States; it has become an important and vital part of the history of the early modern world. As nationhood has receded in importance, historians have become less interested in early America for its own sake and more for what it reveals about the great transformation from pre-modern to modern society. Hence the importance of the slew of articles and books on the origins of capitalism in rural America that have emerged over the past two decades.⁵⁷

Not only is American colonial history today best seen in relation to West European and African history, but it now seems to make sense only as it embraces the peoples of Hispanic America as well.⁵⁸ Whether this hemispheric perspective

⁵³ Joyce Appleby, “A Different Kind of Independence: The Postwar Restructuring of the Historical Study of Early America,” *William and Mary Quarterly*, 3d ser., 50 (1993): 245–67; John Higham, “The Future of American History,” *Journal of American History*, 80 (1994): 1289–1309; Michael McGerr, “The Price of the ‘New Transnational History,’” *AHR*, 96 (October 1991): 1066.

⁵⁴ Appleby, “Different Kind of Independence,” 249.

⁵⁵ Bailyn, *Peopling of British North America*, 3.

⁵⁶ Dixon Ryan Fox, *Herbert Levi Osgood: An American Scholar* (New York, 1924), 72.

⁵⁷ For a review of a number of these works, see Gordon S. Wood, “Inventing American Capitalism,” *New York Review of Books*, 41, no. 11 (June 9, 1994): 44–49.

⁵⁸ Bernard Bailyn, “The Challenge of Modern Historiography,” *AHR*, 87 (February 1982): 2; A.

will be more productive than that suggested by Herbert E. Bolton in the 1930s, only time will tell.⁵⁹ At any rate, some early American historians are calling for entirely new conceptions of the colonial past, new conceptions that would make the history of Santa Fe in 1776 just as important as the history of Boston in 1776. In fact, the Institute of Early American History and Culture in Williamsburg, the center for early American studies in America and the publisher of the *William and Mary Quarterly*, has recently announced that it plans "to diversify its agenda." Without abandoning a traditional commitment to studying the British North American colonies, "Institute publications and programs are currently attempting to embrace a larger range of subjects, including especially the peoples of Hispanic America and West Africa."⁶⁰

Cutting loose from the origins of the nation has allowed not only for a broadening of perspective but for a narrowing as well—studying not the nation or the British Empire or even single colonies but rather towns or counties or even obscure single families. On the face of it, such a narrowing of perspective does violence to everything that the generation of 1895 believed in. What the early American historians of the 1890s disliked above all about the colonial history of their predecessors was its localist, parochial, fragmented, and seemingly trivial character. They scorned the many histories of the individual colonies that had proliferated in the nineteenth century, and they had come to believe that all local history was antiquarianism. As editor of the *AHR*, Jameson turned down numerous articles on the grounds that they were not general or national enough in scope.⁶¹ Now all this earlier cosmopolitanism has been inverted.

Much recent history writing about early America has involved a turning inward to the private spaces and personal lives of ordinary people—exploiting in particular the insights of anthropology. Historians began in 1970 by reconstructing the New England colonial towns as stable, tightly knit, homogeneous, profoundly religious, almost peasant-like pre-modern communities. From these community studies, it was a short step to historians' putting together the *mentalités* of these ordinary folk and recovering their distinctive ways of thinking. At the same time, historians have become increasingly eager to recover the voices of people who had hitherto been ignored by the traditional historiography. Most important of these were the voices of women; and, in the past two decades, historians such as Linda Kerber, Mary Beth Norton, Laurel Thatcher Ulrich, and others have begun the difficult task of reconstructing the lives of women who left no traditional sorts of records.

By the 1990s, the landscape of early American history had been radically transformed. With the ever-multiplying numbers of studies on all aspects of early American society—counties, towns, families—the concept of "society" no longer

Roger Ekirch, "Sometimes an Art, Never a Science, Always a Craft: A Conversation with Bernard Bailyn," *William and Mary Quarterly*, 3d ser., 51 (1994): 657.

⁵⁹ Herbert E. Bolton, "The Epic of Greater America," *AHR*, 38 (April 1933): 448–74.

⁶⁰ James A. Hijiya, "Why the West Is Lost," *William and Mary Quarterly*, 3d ser., 51 (1994): 276–92; Forum, "'Why the West Is Lost': Comments and Response," *ibid.*, 717–54.

⁶¹ Morey D. Rothberg, "'To Set a Standard of Workmanship, and Compel Men to Conform to It': John Franklin Jameson as Editor of the *American Historical Review*," *AHR*, 89 (October 1984): 962–63, 971.

seemed a viable subject for analysis. So detailed has our understanding of the various bits and pieces of society become, so numerous are the monographs on this group or that local community, that historians have steadily lost confidence in their ability to conceive of early American society or even the society of a single colony as the proper arena for study. The consequence is ever smaller and more intimate snatches of history, some of which, like Ulrich's study *A Midwife's Tale* and John Demos's book *The Unredeemed Captive*, are truly marvelous to behold.⁶²

Called at various times microhistory or ethnographic history, this kind of intimate history takes small events in the past involving inconspicuous people and a limited number of sources and teases out of them stories and meanings that presumably throw light on the larger society. Although Italian scholars have had a powerful influence in creating this genre of history writing, early American scholars who write versions of microhistory probably have been most influenced by ethnography and Clifford Geertz's "thick descriptions" of ordinary events. Ethnography, which began as the study of alien peoples, seems ideally suited for the study of an alien past. Certainly, Rhys Isaac's reconstruction of a lost eighteenth-century Virginia world beautifully demonstrates what an ethnographic approach can do.⁶³ Yet traditional ethnography also has been ideally suited for the study of the native Indians.

Precisely "because the colonial period," as Joyce Appleby reports, "was attracting historical curiosity independent of interest in American nationhood," it was now possible for the Indians to become an object of mainstream historical investigation. Preoccupied with the roots of the United States and its institutions, the historians of the 1890s had scarcely acknowledged the existence of the native American peoples. In the opening paragraphs of his essay in the first issue of the *AHR*, Frederick Jackson Turner set forth his entire thesis for understanding the origins of the United States, and the Indians had no place in it. For Turner, the New World that the Europeans came to in the seventeenth century was "virgin soil," an "unexploited wilderness" out of which American distinctiveness was born; it was "the fact of unoccupied territory in America that sets the evolution of American and European institutions in contrast."⁶⁴

No colonial historian could write that way anymore. Through the efforts of recent historians, the Indians have made their presence in early America felt with a vengeance. During the past decade or so, the number of books and articles on the native peoples of North America in the colonial period has grown dramatically. In the fifteen years of the *William and Mary Quarterly* between 1959 and 1973, only four articles on Indians appeared. But in the fifteen years between 1974 and 1988, there were twenty articles dealing with native Americans. And since 1988, the number of contributions to what has come to be called "ethnohistory" has increased even faster. Indeed, as Ian K. Steele has recently written, the "field of ethnohistory . . . is developing so quickly that any attempt at accessible synthesis

⁶² Laurel Thatcher Ulrich, *A Midwife's Tale: The Life of Martha Ballard, Based on Her Diary, 1785–1812* (New York, 1990); John Demos, *The Unredeemed Captive: A Family Story from Early America* (New York, 1994).

⁶³ Rhys Isaac, *The Transformation of Virginia, 1740–1790* (Chapel Hill, N.C., 1982).

⁶⁴ Turner, "Western State-Making in the Revolutionary Era," 70–72.

is bound to be premature and incomplete.”⁶⁵ Some of the best and brightest historians in the country have been turning to the Indians as a topic of research, and books on Indians in early America have begun winning prestigious prizes.

While many of these historians made no effort to twist their accounts of the native peoples into part of the mainline history of the United States, others have sought to make their studies relevant to the roots of the nation. Thus James Axtell contended that the presence of the natives was “essential . . . to the exploration, colonization, and national origins of America.” “Without the Indians, America would not be America as we know it.”⁶⁶ Axtell’s case is a good one, and through studies such as Steele’s *Warpaths*, we are coming to appreciate just what a barbarous and violent place early America was. But some anthropologists have gone further and have become determined to show that the Indians made significant contributions to America’s political institutions, including the idea of federalism and the making of the Constitution. Some have even gone so far as to contend that “democracy” was “perhaps Native America’s greatest contribution to the world.” It “toppled European monarchies and ultimately resulted in the formation of the United States.”⁶⁷

The squabble recently created by this tracing of supposed Indian influence on American democracy demonstrates that the desire to discover the roots of the United States still runs strong, even among anthropologists and Native Americans, and that early American history is as important and vital to people today as it was a hundred years ago. Early America still seems to be the place where we Americans most readily get a bearing on where we have come from and what kind of people we are. Despite the sometimes gross presentism of much current writing about early America, our historiography still yearns to be as objective and as true to the past as the historiography of 1895 yearned to be, and most present-day historians still judge each other’s works in accord with the professional standards developed a century ago. The ever-growing and flourishing state of early American historiography suggests that the dreams of the generation that launched the *AHR* have been more than fulfilled. It is the cynical and jaded Henry Adams of 1895 who was wrong about the future of American history writing.

⁶⁵ Ian K. Steele, *Warpaths: Invasions of North America* (New York, 1994), preface.

⁶⁶ James Axtell, ed., *The Indian Peoples of Eastern America: A Documentary History of the Sexes* (New York, 1981), xvi.

⁶⁷ Oren Lyons, et al., eds., *Exiled in the Land of the Free: Democracy, Indian Nations, and the U.S. Constitution* (Santa Fe, N.Mex., 1992), 2. This volume contains a selection of articles by scholars and Native American leaders promoting the contribution of the Indians to American democracy and the U.S. Constitution. For debate over the issue, see Elisabeth Tooker, “The United States Constitution and the Iroquois,” *Ethnohistory*, 35 (1988): 305–36; and Bruce E. Johansen and Elisabeth Tooker, “Commentary on the Iroquois and the U.S. Constitution,” *Ethnohistory*, 37 (1990): 279–97. I owe these references to Prof. Shepard Krech III.

Turnerians All: The Dream of a Helpful History in an Intelligible World

PATRICIA NELSON LIMERICK

I prefer to believe that man is greater than the dangers that menace him; that education and science are powerful forces to change these tendencies and to produce a rational solution of the problems of life on a shrinking planet.

Frederick Jackson Turner, 1924¹

WHEN YOU ENTER AN ESSAY by Frederick Jackson Turner, you enter an enchanted world. In this world, abstractions are tangible and virtually animate, right on the verge of speaking for themselves. Conditions, forces, ideals, institutions, traits, types, elements, and processes inhabit the Turnerian world like the weightiest and most settled of citizens. If you bump into a social force or a pioneer ideal, it will be you who gets the bruise. You can, of course, try to refuse the impact. "Neither a 'social force' nor a 'pioneer ideal,'" you can declare, with accuracy on your side, "is a real thing. Both are intellectual constructions. Neither should have the power to bruise."

As they have for nearly a century, Turner's conditions, forces, ideals, institutions, traits, types, elements, and processes remain undissolved by such a challenge. You are free to show, at length and in detail, that these concepts exist without the support of much evidence. You will still have to walk around them. After a century of holding their ground, they are not going to disappear in response to the latest frail protest against their power.

Most enduring of all his hardy mental constructions, Frederick Jackson Turner's Frontier Thesis entered its second century in remarkably good shape. First presented in 1893 as "The Significance of the Frontier in American History," the longevity of the Frontier Thesis has made it a historic relic itself, an important and instructive artifact of the American past. When John Mack Faragher referred to Turner's 1893 essay as "the classic expression of the 'frontier thesis,'" his phrasing called to mind the special provisions many states offer to owners of old cars.² At a certain vintage, aged automobiles receive the title of "classic" and gain an exemption from emissions tests. In a similar manner, the Frontier Thesis has become a classic, exempted from the usual tests of verification, evidence, and accuracy. While other historical models are stopped and inspected at every

¹ Frederick Jackson Turner, "Since the Foundation [of Clark University]," in Frederick Jackson Turner, *The Significance of Sections in American History* (1932; rpt. edn., Gloucester, Mass., 1959), 234.

² *Rereading Frederick Jackson Turner: The Significance of the Frontier in American History and Other Essays*, with commentary by John Mack Faragher (New York, 1994), 1.

checkpoint and at many sites in between, the Frontier Thesis operates with a different license entirely.

If one doubts the staying power of this historic model, one has only to remember that it did not lose momentum despite a devastating set of critiques in the 1930s and 1940s.³ Perhaps most remarkably, the Frontier Thesis kept running even when it collided with the full force of Richard Hofstadter's critical intelligence, in his book *The Progressive Historians*, in 1968. When Hofstadter was through with it, if the outcome had been governed by the usual set of historiographic rules, the Thesis would have been finished. Consider just a few items from Hofstadter's catalog of the significant elements of westward expansion that received little or none of Turner's attention in the Thesis:

the careless, wasteful, and exploitative methods of American agriculture; . . . the general waste of resources and the desecration of natural beauty; the failure of the free lands to produce a society free of landless laborers and tenants . . . ; the rapacity and meanness so often to be found in the petty capitalism of the new towns; . . . the crudeness and disorder, the readiness to commit and willingness to tolerate violence; the frequent ruthlessness of the frontier mind, to which Indians, Spaniards, and Mexicans could testify and which had its repeated reverberations in national policies; the arrogant, flimsy, and self-righteous justifications of Manifest Destiny engendered by American expansionism.⁴

A model so riddled with holes could explain only a fraction of the "frontier" story, and Hofstadter did not hold back in sketching the scale of these omissions. Characteristic of his bluntness and refusal to pull punches was his appraisal of Turner's casting of Thomas Jefferson as a typical, democratic product of the frontier. "[A]lmost everything Turner says about Jefferson," Hofstadter wrote, "is either badly nuanced, a misleading half truth, or flatly wrong."⁵ After 1968 and the publication of Hofstadter's *Progressive Historians*, there was every reason to recognize that Turner's Frontier Thesis existed in its own bewitched historiographical space, a zone in which critiques and contradictory evidence instantly lost power and force. Fighting the thesis was like fighting the Pillsbury Dough Boy; it bent momentarily to absorb challenges and then instantly resumed its previous shape.

A greater awareness of my predecessors' experience with this less-than-satisfying combat could have saved me considerable time and trouble. Ten years ago, I was convinced that the revitalization of western American history began with the recognition that the Frontier Thesis had become entirely irrelevant to the history of the Trans-Mississippi West.⁶ But this campaign to reenergize western history turned out to yield a wonderfully ironic side effect. The New Western History's campaign to declare Turner irrelevant revitalized Turner's reputation. It resulted

³ Stephen C. Sturgeon, "Where Seldom Is Heard a Discouraging Word: The Failure of the First Anti-Turner Rebellion, 1930–1945," unpublished manuscript.

⁴ Richard Hofstadter, *The Progressive Historians: Turner, Beard, Parrington* (New York, 1968), 147–48.

⁵ Hofstadter, *Progressive Historians*, 133. See also Richard Hofstadter, "Turner and the Frontier Myth," *American Scholar*, 18 (1949): 433–43.

⁶ Patricia Nelson Limerick, *The Legacy of Conquest: The Unbroken Past of the American West* (New York, 1987); Patricia Nelson Limerick, Clyde A. Milner II, and Charles E. Rankin, eds., *Trails: Toward a New Western History* (Lawrence, Kan., 1991); Richard White, "It's Your Misfortune and None of My Own": *A History of the American West* (Norman, Okla., 1991); James R. Grossman, ed., *The Frontier in American Culture: Essays by Richard White and Patricia Nelson Limerick* (Berkeley, Calif., 1994).

in the name "Frederick Jackson Turner" being featured in stories in the *Washington Post*, the *New York Times*, *U. S. News & World Report*, the *New Republic*, and many other newspapers and magazines. It restored his celebrity. It allowed his followers and supporters to say that his argument in "The Significance of the Frontier" must be very worthwhile and important if it was still worth attacking a century later. The more forcefully one said that Turner was wrong, the more one provided in the way of well-publicized opportunities for his admirers to praise him.

If one might put this in Turnerian terms, a person who found herself placed in an environment so saturated with irony was a person who had two choices: to adapt to that environment and develop the appropriate traits for survival or to retreat to a less challenging environment. The traits of successful adaptation, in this case, were not the pioneer traits of individualism and self-reliance but the post-pioneer traits of a sense of humor and a willingness to reappraise one's relationship to Turner. The sense of humor was surprisingly easy to come by; only the congenitally humorless could have failed to be amused by the late twentieth-century dynamics of Turner's restored visibility. Without the occasion of the *American Historical Review's* centennial, however, one could have controlled one's impulse to rethink one's assumptions and reexamine the patriarch. Given Frederick Jackson Turner's omnipresence in the early issues of the *AHR* and his central role in the shaping of the historical profession in the United States, reassessing his work became both an obligation and an opportunity. In this project, recent squabbles over the appraisal of western American history become blessedly insignificant. The issue becomes, instead, the much more consequential one of Turner's demonstration of the perils and privileges of being a historian. Central to that demonstration is this mystifying fact: in a century of challenges to the 1893 thesis, no historian put forth a more telling challenge than did Frederick Jackson Turner himself.

THE AUTHOR OF THE FRONTIER THESIS, in other words, was also the author of the Frontier Antithesis. The two bodies of ideas came with a considerable difference in form of presentation: the Thesis appeared in one dedicated essay, while the Antithesis appeared in scattered sentences and paragraphs in essays written and published before and after 1893. It is a telling fact that Turner never brought the Antithesis together into one essay; in truth, he never remarked on how thoroughly his own reflections elsewhere had eaten the ground out from under his Thesis of 1893. And yet, if one brings together these dispersed remarks, the results reveal the clear outline of a forceful Frontier Antithesis.

An appreciation of the Frontier Antithesis requires a brief review of the characteristics of the Thesis itself. In casting the frontier as the most important factor forming American character and democracy, the 1893 essay paid little attention to Indians. It stressed the individualism and self-reliance of the pioneer and had correspondingly little to say about federal aid to expansion. It concentrated on the history of the humid Middle West to the neglect of the arid West beyond the 100th meridian. Perhaps most consequentially, it provided support for

models of American exceptionalism by emphasizing the uniqueness of the American frontier experience. The following paragraphs, a composite of direct quotations with a few interludes of paraphrasing, present Turner's challenges to his own claims from 1893.

The Frontier Antithesis by Frederick Jackson Turner
with the assistance of Patricia Nelson Limerick

In the history of Indians lies fresh and important territory for exploration. Scholars are now uncovering "evidences of the rise and fall of Indian cultures, the migrations . . . , hinted at in legends and languages, dimly told in the records of mounds and artifacts, but waiting still for complete interpretation." As one indication of the natives' importance, in various areas of North America, "the Indians had burned over large tracts," creating a new landscape before European arrival. The patterns of trade between and among Indians shaped the fur trade between whites and Indians. "It was on the foundation, therefore, of an extensive intertribal trade that the white man built up the forest commerce." "Another set of problems which need study is to be found in the effect of the Indian on our political institutions." "There is needed a study of our relations to the American Indian" because this topic "is an exceedingly important one in the history of American development, and one from which rich results may be expected." "The native populations have . . . been determining factors in our development."⁷

In the Trans-Mississippi West, "the systematic slaughter of millions of buffalo . . . destroyed the economic foundation of the Indians. Henceforth they were dependent on the whites for their food supply." The Far West offered compelling evidence that the pioneers, like the Indians, found themselves in a state of dependence. When "the arid lands and the mineral resources of the Far West were reached, no conquest was possible by the old individual pioneer methods. Here expensive irrigation works must be constructed, cooperative activity was demanded in utilization of the water supply, capital beyond the reach of the small farmer was required." Individual effort and enterprise were no longer sufficient; thus "the West has been built up with borrowed capital." "Railroads, fostered by government loans and land grants, opened the way for settlement . . . ; [t]he army of the United States pushed back the Indian, rectangular Territories were carved into checkerboard States." States such as these did not arise from the usual evolutionary processes of frontier democracy; they were, instead, sudden "creations of the federal government." In many ways, "the later frontiersman leaned on the strong arm of national power." Most recently, "the government has taken to itself great areas of arid land for reclamation by costly irrigation projects . . . The government supplies the capital for huge irrigation dams and reservoirs and builds them itself." "By the conditions on which [the federal government] disposes of the land and water privileges, it preserves a parental control over the social and economic conditions of the section." In these circumstances, "the pioneer of the arid regions must be both a capitalist and the protege of the government," "adjust[ing] his life to the modern forces of capital and to complex productive processes."⁸

⁷ Frederick Jackson Turner, "The Significance of the Mississippi Valley in American History," in *The Frontier in American History* (1920; Tucson, Ariz., 1986), 179, 89; "The Indian Trade in Wisconsin," in *The Early Writings of Frederick Jackson Turner*, Everett E. Edwards, comp. (Madison, Wis., 1938), 93; "Problems in American History" (1892), in *Early Writings*, 77; "Problems in American History" (1904), in *Significance of Sections*, 18; "Problems in American History," *Early Writings*, 75-76.

⁸ Frederick Jackson Turner, "The Middle West," in *Frontier*, 144; "Contributions of the West to

In the Trans-Mississippi West and elsewhere, international forces were clearly at work in shaping the frontier process. American history was clearly shaped by "world-wide forces of reorganization incident to the age of steam production and large-scale industry." In this case and many others, "ideas, commodities even, refuse the bounds of a nation. All are inextricably connected, so that each is needed to explain the others." Despite the claims of antiquarians, "local history can only be understood in the light of the history of the world." The historian is thus obliged to push his inquiries beyond national boundaries. "If, with our own methods of the occupation of the frontier, we should compare those of other countries which have dealt with similar problems—such as Russia, Germany, and the English colonies in Canada, Australia, and Africa—we should undoubtedly find most fruitful results."⁹

In comparison with the assertive style of the 1893 frontier essay, a number of these remarks may sound tentative—less assertions than calls for new studies and new approaches, really more an *invitation* to an Antithesis than a presentation of the Antithesis itself. And yet each proposition shines a bright light on a central weakness in the 1893 Thesis. In the attention to Indians, to federal involvement in western expansion, to the factor of aridity, and to the desirability of a comparative history that avoided the trap of national exceptionalism, the Turnerian Antithesis anticipated many of the major objections to the Thesis.

Thus the mystery: if Turner had, on his own, identified many of the principal flaws in his earlier position, why did he not say so directly? It would have been a simple matter, and to many an impressive and graceful gesture, to write a later statement like this: "In 1893," he could have said, "nine years before the passage of the Newlands Reclamation Act, I simply did not foresee the expansion of federally subsidized, irrigated agriculture. This, along with my preoccupation with the well-watered Middle West and my personal enthusiasm for the character traits of the frontiersman, led me to underestimate the role of the federal government in western development, and to underestimate, as well, the differences between the frontier process as it worked in the Mississippi Valley and as it worked in the Far West."

The reflective essay was Turner's favorite genre, and retrospect and reappraisal have always been compatible with that literary form. Turner had, moreover, laid the groundwork for this kind of reconsideration. "Generalizations . . . upon the West as a whole," Turner once wrote in a statement that seemed to leave wide maneuvering room for hindsight, "are apt to be misleading."¹⁰ In a habit still very much in evidence among present-day practitioners of his profession, Turner built into many of his articles a number of declarations of modesty, tentativeness, and incompleteness: "Within the limits of this article, treatment of so vast a region, however, can at best afford no more than an outline sketch."¹¹ "I have not the

American Democracy," in *Frontier*, 258; "The Problem of the West," in *Frontier*, 219, 218; "Pioneer Ideals and the State University," in *Frontier*, 278; "Is Sectionalism Dying Away?" in *Significance of Sections*, 310; "Pioneer Ideals," in *Frontier*, 279, 276.

⁹ Frederick Jackson Turner, "Social Forces in American History," *AHR*, 16 (January 1911): 217; "The Significance of History," in *Early Writings*, 57; "Problems in American History," in *Significance of Sections*, 18–19.

¹⁰ Frederick Jackson Turner, "The West—1876 and 1926," in *Significance of Sections*, 236.

¹¹ Turner, "Middle West," in *Frontier*, 127.

temerity to attempt a history of the [Ohio] Valley in the brief compass of this address. Nor am I confident of my ability even to pick out the more important features of its history in our common national life."¹² And, from the 1893 frontier essay, a firm declaration of its own limits: "This paper will make no attempt to treat the subject exhaustively; its aim is simply to call attention to the frontier as a fertile field for investigation and to suggest some of the problems which arise in connection with it."¹³ In theory, these statements left their author a free man, free to reconsider and even free to confess error and second thoughts. Since he had never claimed to have figured everything out, Turner was fully licensed to rethink, reappraise, reconsider, and revise his earlier assertions.

In an essay called "The Old West," Turner made the most handsome and quotable of his humble disclaimers: "The present paper," he wrote, "is rather a reconnaissance than a conquest of the field."¹⁴ The conqueror has to hold his ground and cannot retreat; the man conducting a reconnaissance can always go back, gain new information, and add to or modify the first impressions recorded in his initial report. The passage of time offered this particular historical explorer many chances to rethink his earlier impressions. There were opportunities beyond counting for the older Turner to engage in an instructive dialogue with the younger Turner. And yet, except in the brief and indirect suggestions of the Frontier Antithesis, Turner consistently refused those opportunities.

Why? Here we return to Turner's standing as the representative historian, a central player in the routinizing and professionalizing of the academic historian's activities. Turner's life, Richard White has reminded us, is remarkable for the very elements that now make it seem "commonplace." In a time in which professional roles were becoming defined and institutionalized, "Turner was an academic historian of middle-class origins and progressive sympathies whose professional career centered on university teaching." White's characterization underplays the degree to which Turner felt himself obligated to speak to public audiences; by Turner's definition of the role, neither the historian nor his ideas were to be locked up within the university. And yet, with a broadened definition of the historian's role, White is right in observing that Turner "never seemed to have aspired to be anything more than a professional historian and a university professor."¹⁵

Reading this sentence evokes an unexpected chord of solidarity. Who, one surprises oneself by thinking, would aspire to more? The ambition to be a professional historian and a university professor seems, to a surprising number of people in the 1990s, still to be an appealing and compelling goal. A walk through the mass interviewing area at the American Historical Association meetings will verify how "commonplace" Turner's once-novel ambitions have become.

¹² Frederick Jackson Turner, "The Ohio Valley in American History," in *Frontier*, 160.

¹³ Frederick Jackson Turner, "The Significance of the Frontier in American History," in *Frontier*, 3.

¹⁴ Frederick Jackson Turner, "The Old West," in *Frontier*, 69.

¹⁵ Richard White, "Frederick Jackson Turner," in John R. Wunder, ed., *Historians of the American Frontier: A Bio-Bibliographical Sourcebook* (New York, 1988), 660. White draws this characterization from the telling description of Turner's role in the professionalization of history in Ray Allen Billington, *Frederick Jackson Turner: Historian, Scholar, Teacher* (New York, 1973).

SINCE TURNER WAS, GENUINELY, A FOUNDING FATHER of the profession, the patterns of his life, the workings of his mind, and, especially, the pressures and mandates of his ambition carry lessons far beyond the individual. As a young man, Turner wanted to make an impact. Ray Allen Billington's biography as well as Allan G. Bogue's forthcoming biography make it clear that Turner, as a young man, actively solicited and pursued opportunity and recognition. As John Mack Faragher notes, when the frontier essay appeared in print in 1894, Turner "purchased hundreds of offprints and distributed them widely, not only to colleagues and associates but also to prominent members of the national intellectual elite—a practice he followed with all his publications."¹⁶

When all those offprints paid off and the Frontier Thesis finally became a pillar—some would say *the* pillar—of American history, Turner was triumphant. But he was also trapped: trapped, in part, by a host of impossible book-writing obligations brought to him by the success of the frontier essay, but also trapped by the fact that his career and status now rested on his identification with the 1893 thesis. In a letter in 1919, he referred to the relief he felt upon learning that another historian (who happened to be Woodrow Wilson) made no claims to the origins of the Frontier Thesis. It would have been "unpleasant," Turner said, "to contend for the parentage of my intellectual firstborn, dear as it is to me, and much as it means for my reputation."¹⁷

The phrase "much as it means for my reputation" captures the problem. Turner had wanted very much to succeed, and the favorable reception of the Frontier Thesis was a key part of that strategy for success. Its good standing brought many benefits: the presidency of the American Historical Association, a position at Harvard, many followers and devotees, and a happy package of status, prestige, visibility, and name recognition. But the price of success, it turned out, was a significant one: a surrender of the spirit of open-ended inquiry and forceful assertion that had clearly provided Turner with great satisfaction and pleasure as a young man.

The Originator of the Frontier Thesis had evolved into the Defender of the Frontier Thesis, and defending would offer little of the fun and adventure that came with originating.¹⁸ One of the pleasures now off-limits to Turner was that of pointing out the short-sightedness and mistaken preoccupations of established authority in American history. When one *was* the established authority in one's field, the pleasures of rebellion were considerably diminished. Turner could not frankly discuss the flaws of the 1893 essay because it formed the foundation of his career, his prestige, and his sense of personal achievement.

His choice not to do so set a precedent from which the profession has never fully broken free. Turner was a historian of great influence, and a graceful and dignified reappraisal of his earlier claims would have set a precedent of great

¹⁶ Faragher, *Rereading Turner*, 3.

¹⁷ Frederick Jackson Turner to William E. Dodd, October 7, 1919, in Ray Allen Billington, ed., *The Genesis of the Frontier Thesis: A Study in Historical Creativity* (San Marino, Calif., 1971), 190.

¹⁸ The use of the word "originating" here should not be understood as an assertion of the Turner Thesis's "originality" as a cultural artifact. For what remains the best survey of the ways in which Turner incorporated preexisting ideas, see Henry Nash Smith, *Virgin Land: The American West as Symbol and Myth* (Cambridge, Mass., 1950).

value. At a time when the conventions, requirements, rituals, and traditions of being a historian were still fluid and up for definition, his decision to withhold public reconsideration was a consequential one.

Given Turner's repeated declarations that our understanding of the past must adapt to changes in the present, it was also a deeply ironic choice. In 1891, Turner had published an essay called "The Significance of History," a forceful and earnest declaration of the unbreakable tie between past and present. In a sentence he himself underscored, Turner declared his faith in presentism: "*Each age writes the history of the past anew with reference to the conditions uppermost in its own time.*"

The aim of history, then, is to know the elements of the present by understanding what came into the present from the past. For the present is simply the developing past, the past the undeveloped present . . . The antiquarian strives to bring back the past for the sake of the past; the historian strives to show the present to itself by revealing its origins from the past. The goal of the antiquarian is the dead past; the goal of the historian is the living present.¹⁹

The true Turnerians, one could conclude from this memorable passage, were the historians who paid attention to, and put into practice, Turner's 1891 declaration of the need for writers of history to rethink and revise. The "Turnerians" who stayed loyal, instead, to the 1893 Frontier Thesis observed the letter and defied the spirit of Turnerian history. But there was the puzzle again: by this categorization, Turner was both true Turnerian and so-called Turnerian, at once a person paying attention to his times, insisting that historical models must be constantly remodeled in response to changing conditions, and a person repeating old formulae that could no longer fit those changing times.

"What endures" in Turner's legacy, John Mack Faragher has argued, "is Turner's commitment to contemporary knowledge, his understanding that debates about the past are always simultaneously debates about the present. It marks him as America's first truly modern historian."²⁰ It marks him, in addition, as an innocent. Orienting a historical inquiry in terms of the present, Turner assumed in 1891, added clarity to one's understanding. One could, the theory went, get a grasp on the fundamental patterns of one's own times. Figuring out the present was an undertaking much like reading the last pages of a novel; one checked to see how the plot one had been following in the earlier pages finally turned out. In examining events in one's time, one applied the same kind of pattern-finding to the present that one applied to the past; and, when one had identified the outcome, or the past's product, one was in a much improved position to trace and understand the whole story, primarily because one could now identify the path of significant change, distinguishing the consequential events from the inconsequential. It was as clear and linear a matter as if the movement from the past to the present were a railroad track. Once one had identified and inspected the point of arrival, it was an easy matter to follow the track back to its origin.

¹⁹ Turner, "Significance of History," in *Early Writings*, 52–53. William Cronon, "Turner's First Stand: The Significance of Significance in American History," in Richard Etulain, ed., *Writing Western History: Essays on Major Western Historians* (Albuquerque, N.Mex., 1991), offers the best discussion of Turner's essay "The Significance of History."

²⁰ Faragher, *Rereading Turner*, 4.

But what if this destination proved difficult to identify? What if the present proved unintelligible, turning out to be even more confusing than the past? Here is where Turner's experience becomes most instructive for his successors in this profession. For reasons that every historian can empathize with, Turner betrayed the doctrine of presentism, and, in turn, the doctrine of presentism betrayed him.

How did he betray it? Like all beliefs, presentism confronts its believers with an irritating challenge: once one has declared one's faith, one is supposed to practice it. That seems easy enough, until the passage of time complicates matters. In 1891, at age thirty-one, Turner recorded his faith in presentism. In 1910, at age fifty, in his Presidential Address to the American Historical Association, he reconfirmed his 1891 declaration, but he referred now to "the familiar doctrine that each age studies its history anew and with interests determined by the spirit of the time."²¹ The very phrase "familiar doctrine" should have been its own warning; "familiar doctrine" was, by definition, unlikely to represent a full reckoning with the cutting edge of the present.

In 1891, Turner had been a Young Turk, full of vigor, ready to challenge the powers-that-be. In 1910, he was president of the American Historical Association, fighting off the challenges of the next cycle's crop of Young Turks, themselves now ready to challenge the powers-of-which-Turner-had-become-one. "*Each age writes the history of the past anew, with reference to conditions uppermost in its own time,*" and 1910 clearly offered a different set of conditions from 1893. It was time for a vigorous reappraisal of the 1893 Frontier Thesis, or so one might have thought if one were not oneself the author of that much-admired and frequently cited "familiar doctrine."

Like many rebels done in by the success of their rebellion, the middle-aged Turner reneged on the proclamations of freshness and innovation he had made as a young man. But this was a two-way betrayal: if Turner had betrayed his declaration of faith in presentism, presentism had also betrayed him. The present, remember, was supposed to illuminate the past, not befog it; the present was supposed to help the historian's undertaking, not frustrate and obstruct it. The present, however, broke its half of the agreement by proving to be of no help at all in understanding the past. In the first decades of the twentieth century, the present was turning out to be *more* confusing than the past. If one could not figure out the present, and if one's project of explaining the past required one to identify the outcome that the past would then explain, then one had landed, definitively, in a muddle.

Turner knew a lot about the present; he had fulfilled his half of the bargain by keeping up with current events. In the pieces and parts of his Frontier Antithesis, Turner referred to post-1890 booms in petroleum, coal, copper, and irrigation, to the concentration of capital, and to capital's struggles with labor in the early twentieth-century West. A presentation in 1924 gave an impressive demonstration of his success in keeping up. Speaking at Clark University, Turner gave an overview of the thirty-five years since Clark's founding. "Few epochs in history," he

²¹ Turner, "Social Forces," *AHR*, 225.

said, "have included such startling changes within a single generation as that between the [1880s] and the present." He called attention to the impact of the movies and radio, of the automobile and the airplane. He remarked on the change in women's work roles produced by the introduction of the typewriter to offices. He mentioned with concern the American distribution of wealth: 2 percent of the population owning three-fifths of the property, a fact that was "food for reflection on the outcome of the pioneer democracy that sought equality as well as opportunity in the American wilderness." Turner referred to revolutions in scientific understanding, mentioning the Einsteinian revolution of relativity and the exploration of the atom. He noted the shift of American action and attention to the Pacific basin. When he spoke of the problem of the planet's growing human population, he sounded as if he were writing an early draft for a late twentieth-century Earth Day rally: "Truly a shrinking earth! An earth compelled by irresistible forces to exercise restraint, to associate, agree, and adjust, or to commit suicide."²²

"Already," Turner wrote in 1925, responding to the most recent statistical information, "the urban population exceeds the rural population of the United States." Fundamentally, Turner knew that the present of the early twentieth century meant urbanization, industrialization, and the consolidation of capital. "The world," Turner said, "has never before seen such huge fortunes exercising complete control over the economic life of a people." In his favorite region, the Middle West, this transformation was unmistakable: "The world has never seen such a consolidation of capital and so complete a systematization of economic processes."²³

But how on earth was one to connect decentralized, rural frontier origins to centralized, industrial outcomes? The easiest option, though it removed most of the punch from the narrative, was to assert, and reassert, and assert once again, that it was the disappearance of those rural frontier conditions that generated the centralized, industrial outcomes. While this strategy carried some logic, it had the disadvantage of a fractured narrative. The first half of the story was the direct opposite of the second half of the story; the past was the frontier, and the present was the factory, and the appearance of the factory coincided with the disappearance of the frontier; Part Two followed on Part One by canceling Part One out. And yet the Turnerian theory of presentism would have required a narrative that told the origins of the factory, tracking the foreshadowings of the consolidation of capital, noticing the hints and clues of the shift from the agrarian and rural to the industrial and urban. Instead, loyalty to the 1893 thesis bound Turner to a narrative in which the frontier kept its position at center stage, and the factory thus emerged suddenly, unexplained, from the wings.

Another option was simply to assert that these two phases were connected and, somehow or other, mutually illuminating. "Seen from the vantage-ground of present developments," Turner declared with exuberance, "what new light falls upon past events!" "When we recall the titanic industrial power that has centred

²² Turner, "Since the Foundation," *Significance of Sections*, 207, 220–21, 234.

²³ Frederick Jackson Turner, "The Significance of Sections in American History," in *Significance of Sections*, 35; "Social Forces," *AHR*, 222; and "Middle West," in *Frontier*, 153.

at Pittsburgh," Turner offered as his illustration, "Braddock's advance to the forks of the Ohio takes on new meaning." Written more logically, the sentence might read, "When we recall the huge industrial power that has centered at Pittsburgh, Braddock's advance to the forks of the Ohio seems oddly irrelevant, strangely unrelated to the manufacturing of steel that has transformed Pittsburgh in our time." But Turner's faith in the connection between past and present required Braddock to have not a new irrelevance but a new meaning. Namely: "[e]ven in defeat, he opened a road to what is now the centre of the world's industrial energy."²⁴ Braddock, it is clear at this point, had not entirely benefited from the gospel of presentism; he did considerably better for himself when his wilderness battles represented an important prelude to the American Revolution than when he came to serve as the not-very-convincing advance scout for the steel industry.

The problem came to center on one of Turner's most valued terms, "ideals." The frontier had produced pioneer ideals, and pioneer ideals were the key to American character and democracy. Thus the often-noted anxiety lodged at the center of the Frontier Thesis: if the frontier had ended, as Turner said it had, then what force could keep those crucially American pioneer ideals alive and vital? "Long after the frontier period of a particular region of the United States has passed away," Turner tried to convince himself, "the conception of society, the ideals and aspirations which it produced, persist in the minds of the people." The conditions, Turner realized, had changed dramatically: "The solitary backwoodsman wielding his ax at the edge of a measureless forest is replaced by companies capitalized at millions, operating railroads, sawmills, and all the enginery of modern machinery." Somehow, the pioneer ideals of the "solitary backwoodsman" were to continue to govern the operations of heavily capitalized factories, providing continuity between two disconnected phases of history.²⁵

If the persistence of these ideals, long past the disappearance of the conditions that produced them, was the good news, the bad news was that these familiar old ideals could work in support of strange new forces. The "masters of industry and capital," Turner said, "came from" frontier society "and still profess its principles." Connecting the frontier past to the industrial present were these improbable latter-day pioneer idealists: John D. Rockefeller, Marcus Hanna, Claus Spreckels, Marshall Field, Andrew Carnegie, and E. H. Harriman. The frontier, Turner had said in 1893, remakes the colonist. "It takes him from the railroad car and puts him in the birch canoe. It strips off the garments of civilization and arrays him in the hunting shirt and the moccasin." "Before long," the transformed pioneer "shouts the war cry and takes the scalp in orthodox Indian fashion." In asserting a continuity of ideals and character between frontier conditions and industrial conditions, Turner was putting Rockefeller, Hanna, Spreckels, Field, Carnegie, and Harriman through a similar process of transformation. The historian had now supplanted the frontier as the force recostuming these pioneers

²⁴ Turner, "Social Forces," *AHR*, 226.

²⁵ Turner, "Contributions of the West," in *Frontier*, 264; and "Pioneer Ideals," in *Frontier*, 280. See David M. Wrobel, *The End of American Exceptionalism: Frontier Anxiety from the Old West to the New Deal* (Lawrence, Kan., 1993).

in hunting shirt and moccasin; it was the historian's voice that told his audience to see these magnates of power as fundamentally still shouting "the war cry" and taking "the scalp in orthodox Indian fashion."²⁶ While the analogy might have made some sense to those who had been run over by Rockefeller and Hanna in their drives to power, both the war cry and scalping technique had changed character considerably as they evolved into the practices of modern industry.

The mandate of presentism was, meanwhile, standing by, ready to release Turner from these interpretative contortions and stretches. "When we look at these matters in the light provided by present conditions, the frontier proved to be less the main trunk line of history, and more a side path through the forest," Turner could, logically, have admitted if he had chosen to follow the precepts he set forth in 1891. "Industry and urbanization are now clearly revealed as the outcome we are obligated to explain. Now we must re-examine the past to find the origins of these crucial forces of our present. *Each age writes the history of the past anew with reference to the conditions uppermost in its own time.* The Frontier Thesis was fine for its time, but now the times demand the Urban/Industrial Thesis. Antiquarians can try to figure out what became of those pioneer ideals, but historians have more urgent obligations." The opportunity to practice, as well as preach, the gospel of presentism was an open and obvious one, but it was an opportunity with little appeal to Turner.

I WRITE HERE with absolutely nothing in the way of smug, placid, distanced judgment of Frederick Jackson Turner. On the contrary, it would have to be said that, in matters of presentism, I have come to "share" Turner's "pain." For years, I applauded, and quoted with enthusiasm, every declaration Turner ever made on behalf of letting our knowledge of the present and our knowledge of the past illuminate each other. The first words that appeared in my book *The Legacy of Conquest* were the words I have quoted here from "The Significance of History."²⁷ But, like Turner, I, too, found out that presentism was a faith with an unbreakable and unappealing habit of turning on its believers.

It was not that I wanted my own Frontier Antithesis, *The Legacy of Conquest*, to stand for the ages. The deference shown to Turner by many western historians had given me an instructive demonstration of the perils of life in a field in which one dominant set of ideas overstayed its period of usefulness. I wanted western American history to have many historians offering, as Turner had, forceful statements and interpretations. If we were lucky, these historians would show a little more flexibility in reappraising their earlier proclamations, and their successors would show a little more willingness to declare their independence of the matriarchs and patriarchs of the field. Under those terms, the best I could hope for my own model of the West was to see it become a stimulant, a provocation, the equivalent to the side of a swimming pool from which future inquiries could push off to gain momentum.

²⁶ Turner, "Contributions of the West," in *Frontier*, 264; and "Significance of the Frontier," in *Frontier*, 4.

²⁷ See Limerick, *Legacy of Conquest*, 17.

Swerving around the dilemma of wanting my work to stand for the ages, I was able to bring all the more momentum to my head-on crash with presentism's much more serious dilemma. As Turner knew and as I now know (and it seems to have caught us both equally by surprise), the present turns out to be even more complicated, more muddled, and more maddening than the past. A historian who goes around declaring that an understanding of the present will deepen our understanding of the past is a historian who will eventually have to eat crow.

The 1990s have served me a generous helping of this unattractive substance, a meal that honesty requires me to consume in public. In the mid-1980s, I was quite sure I had figured out the western American present and equally sure that I knew how to apply that understanding to history. Recent events and trends, I thought, spoke very clearly: the collapse of the most recent boom in oil and coal; the campaign to cut the federal budget and to reduce the subsidies that had supported much of the western economy; an economic downturn in the classic rural enterprises of farming, ranching, mining, and timber; the inescapable dilemma posed by the accumulation of nuclear contamination at western weapons production plants; the visibility of a number of politicians willing to talk about the inability of western resources to support an endlessly expanding population; the reassertion of Indian rights to land, water, religious freedom, and sovereignty; and the upsurge in immigration originating in Latin America and Asia. With all these changes under way, the pattern of the western present and future would simply have to be one of intelligent reckoning and reorientation. Great human diversity was an inarguable quality of the western past and present; white westerners would now have to face up to this central quality of their region. The limits of resources and the instability of traditional western rural economies were now crystal clear; westerners would have to revise their most basic strategies of water use and land use. While Turner had told us that the frontier ended in 1890, nineteenth-century attitudes toward nature and toward people of color had lasted all too vigorously into the twentieth century. But now, one hundred years after that ostensible end of the frontier, the shift was actually happening—away from short-term extraction toward long-term and permanent residence and inhabitation, away from hostile dismissals of minorities toward a recognition that we would all have to live together.

Readers are entitled to think that my effort to write a happy ending to western history makes Turner's efforts to find pioneer ideals at work in monopolistic industrialists look hardheaded and realistic. Certainly, clues began to appear immediately, indicating that presentism was not going to be any kinder to me than it had been to Turner. Of all those clues, the 1994 elections provided the body blow. The hearty vote in favor of California's Proposition 187, denying services and benefits to illegal aliens and their children, suggested that the West was not embarked on a new path of tolerance and amicable coexistence. Western voters, moreover, seemed to be saying a vigorous "NO" to the public lands reforms undertaken by President Bill Clinton's secretary of the interior, Bruce Babbitt. Babbitt earnestly pitched into the effort to reduce the powers of the "lords of yesterday," the legal scholar Charles Wilkinson's fine phrase for the federal resource policies created in the late nineteenth century and still governing the

West today.²⁸ Babbitt's efforts provided the best evidence I had for my theory of an approaching big change in the terms of Western life. But voters in the congressional elections of 1994 seemed to take it as a personal mission to expose my failures in prophecy. As the new Congress took office and the reenergized Republicans undertook to reinstall the lords of yesterday as the lords of today and tomorrow, I found my affection for Turner's 1891 essay, "The Significance of History," diminishing daily.

Along with disappointment, however, came relief, and a kind of relief that Turner may not have permitted himself to feel. When I contemplated my failure to figure out the present, I began to wonder how I had ever fallen into the belief that such an achievement was within my reach, or within my job description. And, happily, as disorientation with the present increased, enthusiasm for studying the past grew proportionately. Compared to the inscrutable present, the past was an open book.

Disillusionment with presentism had the happy side effect of restoring a recognition of contingency and improbability to my understanding of history. When I thought I was tracing a direct, clear line between past and present, an unhealthy fatalism crept into my thinking. Event followed event with all the drama, surprise, and spontaneity of the prearranged program of the Rose Parade. Losing control of the pattern of present events thus offered its own form of liberation; it was as if the floats of that regimented parade suddenly started taking inexplicable right, left, and U-turns, with the General Electric float suddenly slamming into the California Fruit-Growers' entry, filling the air with a rainbow of chrysanthemums and marigolds and giving the poor commentators something, at long last, to comment on. History became correspondingly more interesting as it became more chaotic.

Here, too, Turner's struggles proved to be emblematic and precedent-setting. Turner, like many of his fellow participants in the founding of his profession, wanted to establish some control over the past. He wanted to persuade the disorderly events and people of history to submit to some kind of order. The agents and devices he used to produce that order were those reified and nearly animate concepts he stayed loyal to all his life: the conditions, forces, ideals, institutions, traits, types, elements, and processes that could translate the chaos of history into patterns of clarity and manageability.

As with Turner's presentism, a familiar form of historian's hopefulness was at work here. With its powers of analysis, Turner thought, the mind could diagram the working parts of human history and then show how the parts fit together. Turner's hunger for the ideas that would hold a fragmented world together was palpable and urgent. Despite his ritual declarations of tentativeness and modesty in his essays, his heart clearly melted when in the company of a rhetorically convincing generalization, with abstractions and evidence marching together in a pleasant and ordered parade toward the present.

Success in this venture, though, carried a great price: the more effectively Turner identified and arranged the determinants of history, the more he

²⁸ Charles F. Wilkinson, *Crossing the Next Meridian: Land, Water, and the Future of the West* (Washington, D.C., 1992).

sacrificed drama, as well as realism. The complexity of historical evidence was silenced and squeezed in order to fit into the tight categories Turner built for it. "Each type of industry," Turner had said in one of his best-known passages of fatalistic thinking, "was on the march toward the West, impelled by an irresistible attraction." "Each passed in successive waves across the continent. Stand at Cumberland Gap and watch the procession of civilization, marching single file—the buffalo following the trail to the salt springs, the Indian, the fur-trader and hunter, the cattle-raiser, the pioneer farmer—and the frontier has passed by."²⁹ A historical procession led by a large herbivore in search of salt was unlikely to provide the material for a celebration of the human will and the power of individual choice. And when this parade, in the next sentence, picked itself up and reappeared to march, in identical order, through South Pass in Wyoming, a large share of the contingency, improbability, and interest of historical change seemed to have been surrendered.

Identifying types, forces, and conditions, Turner could write with a tone of certainty that bordered on the theological. In his historical writings, Turner virtually never referred to God. Even his discussion of religion as a factor in history never got beyond the perfunctory. And yet, in his declarations on the patterns of history, Turner nonetheless wrote in a tone of certainty that conveyed a secularized faith in destiny. A controlling and predestining God might not be visible in the picture, but forces, ideals, institutions, traits, types, elements, and processes were ably filling in for Him.

Without an explicitly religious framework of thought, Turner offered a vision of history that nonetheless seemed destined and predetermined. His unrelenting enthusiasm for hydraulic metaphors gave clear and consistent evidence of his susceptibility to deterministic thinking. Telling the story of westward expansion, one of the most dynamic and complicated stories in history, a story much twisted and inflected by variations in individual and group will, Turner habitually used metaphors that eliminated choice and decision from the picture. People moved in streams, tides, flows, currents, torrents, floods, and inundations. A southern "stream" from Virginia "flowed into Ohio," while "the main current that sought Indiana came from North Carolina"; "[b]y 1830 the Southern inundation ebbed and a different tide flowed in from the northeast"; "[t]he homestead law increased the tide of settlers."³⁰ Here, in these hydraulic metaphors, Turnerian determinism was at its clearest. Whatever individual participants in these movements might have thought they were doing, the historian had, with hindsight, detected and labeled the great sweeps and trends that both caused and explained these individual decisions.

And yet, just as Turner himself challenged his Frontier Thesis with a scattered Antithesis, here, too, in the matter of determinism, Turner vigorously and persuasively contradicted himself. One particular topic brought out in him a different spirit from the one he showed when tracing the workings of destiny in westward expansion. When he wrote about the contest of empires for the interior

²⁹ Turner, "Significance of the Frontier," in *Frontier*, 12.

³⁰ Frederick Jackson Turner, "Dominant Forces in Western Life," in *Frontier*, 224; "Middle Western Pioneer Democracy," in *Frontier*, 346; and "Pioneer Ideals," in *Frontier*, 276.

of North America, a very different and much more persuasive historian seemed to have taken control of his pen.

In two articles in particular, "The Origin of Genet's Projected Attack on Louisiana and Florida" and "The Policy of France toward the Mississippi Valley in the Period of Washington and Adams," contingency came back to overpower determinism. In truth, without an admission of contingency, without a recognition that events of the past did not proceed on a fated march toward the present, the story of the schemes and plans of various empires to gain control of the interior would have been killingly dull and pointless. Describing the visions of persuasion and territorial acquisition that Genet took with him to North America in 1792, Turner deliberately broke free from determinism: "A vast and startling project, indeed! sweeping into a single system the campaigns of France in Europe, the discontented frontiersmen of the Mississippi valley, and the revolutionary unrest that was before long to give independence to Spanish America. The historical possibilities of this great design are overwhelming."³¹ With this remark—indeed, simply with the direct and uncharacteristic use of the word "possibilities"—Turner gave Genet's story a license for vitality. He removed, at least for the moment, everyone's hindsight knowledge that the frontiersmen would remain loyal to the new American nation and that Genet's mission would fail. In the same spirit, his article "The Policy of France toward the Mississippi Valley" restored possibility as well as complexity to the struggles for empire in the American interior. One learns from the record of French maneuverings, Turner noted, "how wide-spread were the forces of intrigue for the Mississippi valley." It was not his intention, in this article, to say that a clear American destiny would render these "forces of intrigue" inconsequential. On the contrary, his last sentence in the article deliberately asserted contingency:

No one who has studied the evidence of long-continued menace to the connection of the west with the rest of the United States . . . can doubt that the danger was a real one, and that a European power might have arisen along the Mississippi valley and the Gulf of Mexico, dominating the interior by its naval force, and checking, if not preventing, the destiny of the United States as the arbiter of North America and the protector of an American system for the New World.³²

With the word "destiny" at the end of this passage, Turner returned to the familiar world of a secularized, nationalistic determinism. But for a surprisingly lengthy interlude in his studies of imperial policy, Turner surrendered his usually firm explanatory structure of forces and conditions, and contemplated, instead, the muddled, complex, and open-ended possibilities of the past.

TEN YEARS AGO, I FELT SURE that Turner's weaknesses were the weaknesses of the Frontier Thesis. Both his failure to convey the contingency of history as well as his

³¹ Frederick J. Turner, "The Origin of Genet's Projected Attack on Louisiana and the Floridas," *AHR*, 3 (October 1897–July 1898): 655.

³² Frederick J. Turner, "The Policy of France toward the Mississippi Valley in the Period of Washington and Adams," *AHR*, 10 (January 1905): 273, 279.

failure to comprehend his own times and to align his historical narrative with the outcome facing him in those times, I thought, had their sources in the errors and omissions of his *Frontier Thesis*. In 1995, his failure seems much less distinctive and peculiar to him and much more an early example of the proposition that the present has a way of turning out to be even more bewildering than the past. Surely, the best demonstration of this proposition has appeared in our own times with the dissolution of the Soviet Union and the end of the Cold War. For diplomatic historians and historians of the Soviet Union, the outcome of this plot had been very clear: the rise of Marxism, the face-off between communism and capitalism, the struggles of the superpowers. While historians might disagree in their interpretations of the historical process that brought us to the present, until 1989 the outcome that historians sought to explain was not in dispute. But, with the collapse of Soviet control over Eastern Europe, and then with the end of Russian communism itself, the narrative of history took a wild and unexpected turn. Surely, the past was still connected to the present, but the connections followed paths that were unexplored and unfamiliar, paths that would lead historians through terrain they had barely noticed in their previous efforts to explain a different, much more intelligible present.

With his faith in his conditions, forces, ideals, institutions, traits, types, elements, and processes, his objective terms of analysis, Frederick Jackson Turner believed in an intelligible world. "[A] generation that does not attempt to consider its recent past," Turner wrote, "is like the merchant who ignores his ledger, the mariner who takes no observations."³³ His assumption, of course, was that the merchant who consulted his ledger and the mariner who took observations would be a merchant and a mariner equipped to decipher their positions in life and who, understanding their recent past, could then make better and more effective plans for their future. Turner believed, as well, in progress, even though the increasingly urban and industrial character of the United States put that belief to a trying test.

By the time I entered graduate school in 1972, the historian's right to a faith in progress was greatly, and appropriately, constricted. To note just one factor, the wars of the twentieth century had knocked the pins out from under the idea of a world in which people, as time passed, learned the lessons of history and behaved with increasing wisdom and benevolence. In the graduate schools of the late twentieth century, the phrase "Whiggish history" was in common usage, deployed to indicate that the speaker or writer was too well trained to believe in the old fantasy of progress, too sophisticated to believe in a world in which the transition from past to present was a transition of improvement.

Just beneath the surface of those declarations of cynicism (and in some ways, protected and insulated by the official code of cynicism) was a faith that matched that of Frederick Jackson Turner. By this faith, historians could be *useful*, and useful in a way that might actually help to produce progress. "A just public opinion and a statesmanlike treatment of present problems," Turner wrote in 1910, "demand that [those problems] be seen in their historical relations in order

³³ Turner, "Since the Foundation," in *Significance of Sections*, 208.

that history may hold the lamp for conservative reform.”³⁴ I myself, along with many of my contemporaries, would have preferred to “hold the lamp” for somewhat less cautious reform, but we certainly had no objection to lampholding *per se*, no objection to the notion that historical understanding might lead to wiser and more benevolent action in our own time. An effort to mock the hope that Turner invested in this proposition runs, almost immediately, into the uncomfortable recognition that many historians who are now in their forties and fifties have matched Turner in hopefulness.

On some issues, most particularly on issues of race, Turner’s ideas were light years away from current thought in American historical circles. Turner was a white American historian who never questioned (indeed, never noticed) the role that his own racial and ethnic identity played in shaping his point of view, and the result was a general flatness of curiosity when it came to the histories of non-whites. Turner declared in 1904 that “a study of the negro is needed,” but he himself made no move in the direction of that study. In fact, the cause of advancing the significance of the frontier seemed to require, in Turner’s judgment, the reducing of the significance of slavery. “[W]hen American history comes to be rightly viewed,” he remarked in the 1893 frontier essay, “it will be seen that the slavery question is an incident,” and barely even that. His references to slavery were always brief, and they were almost entirely focused on slavery as a factor in American politics, as a source of tension between white Americans. The lived experience of being a slave barely registered in Turner’s otherwise omnivorous historical curiosity.³⁵

On an issue such as race, then, the use of the phrase “Turnerians All” to describe the common and persistent condition of historians would be absurd. Turner had, for instance, a well-established habit of referring to various ethnic and national “stocks”: “By [Indian] intermarriages with the French traders the purity of the stock was destroyed and a mixed race produced”; in considering general literacy on the frontier, it was “at its worst where the Mexican stock abounded”; late nineteenth-century immigration represented “the peaceful conquest of the old stock by an international army of workers.”³⁶ In his use of the word “stock,” Turner appears as a relic of a distant era of both history and historiography, relying on a term and a pattern of thought that has little kinship with the views held by most historians writing and teaching today.

And yet, in other matters, especially in definitions of the historian’s professional role and in the benefits to society of vigorous historical inquiry, “Turnerians All” is an appropriate form of professional self-description. “The story of the peopling of America has not yet been written,” Turner said in one of his most quotable declarations. “We do not understand ourselves.”³⁷ Turner’s words carry undiminished, if also differently inflected, force today. Which “selves,” after all, were “we” to understand? Turner’s “we” in this passage was the “we” of the American

³⁴ Turner, “Social Forces,” *AHR*, 226.

³⁵ Turner, “Problems in American History” (1904), in *Significance of Sections*, 18; and “Significance of the Frontier,” in *Frontier*, 24.

³⁶ Frederick Jackson Turner, “The Fur Trade,” *Early Writings*, 167; “The West—1876 and 1920,” in *Significance of Sections*, 252; and “Since the Foundation,” in *Significance of Sections*, 211–12.

³⁷ Turner, “Significance of History,” in *Early Writings*, 64.

people, and not the “we” of the historical profession. The occasion of the centennial of the *AHR* is its own reminder that the self-understanding of historians is an indispensable part of the project of understanding the history of the region, the nation, and the planet. Turner’s unwillingness to scrutinize himself and to put his own earlier, career-establishing ideas through a process of critical examination has become a luxury we can envy but no longer afford. Even if it strikes some traditional historians as oddly and unprofessionally self-indulgent, the project of “understanding ourselves” must include historians’ self-understanding and self-disclosure.

In a widespread misreading of changes in the field of western American history, various critics have noted that Turner, as *New York Times* writer Richard Bernstein put it, “has been scrapped,” dumped from the historical canon because he was “Eurocentric, making the whites the only genuine historical protagonists.”³⁸ By this misinterpretation, then, the New Western History has eliminated Turner because he seems to be “racist” in the terms of the late twentieth century. The point here is, actually, quite different. Turner’s own race, or, more accurately, his society’s construction of the value and meaning of his whiteness, did shape and direct his historical attention. Being white set stern limits on his empathy and understanding. While he was an impassioned advocate of paying attention to the history of people who were not members of elites, his attention rarely strayed from the social history of white people.

Although there was nothing to stop Turner from reading, for example, W. E. B. Du Bois and getting a head start in these matters, the notion of examining the impact and consequences of his own racial identity was a completely foreign concept to him. Under these circumstances, the point cannot be to chide him for a failure of self-consciousness. The point is to learn from his example and to use it as an occasion to ask ourselves, “What are *our* blind spots? What are the elements of our social identity that limit our vision as sternly as racial assumptions limited Turner’s vision?” However we answer these questions, historians of the late twentieth century have not reached a plateau of comfortable self-understanding from which we can look down smugly on Turner’s struggles. In truth, a confession of solidarity—“Turnerians All”—is the place where this exploration begins and ends.

Squabbles over the validity of the Frontier Thesis have distracted us from the more compelling issue raised by Turner’s example. Frederick Jackson Turner embodied the idea of historians as public servants, as scholars whose inquiries into the past could contribute directly and concretely to human well-being in the present. The familiar dispute—Was Turner right about the frontier?—has become trivial and arcane. The much less familiar question—Was Turner right about the present-day value of historical understanding?—remains the question of consequence. In everyday life, that question has been pushed to the edge of our attention by the constant round of activities of the professional historian, activities rendered normal by the careers of Turner and his contemporaries: class-preparing, lecture-giving, discussion-leading, paper-grading, student-advising, ar-

³⁸ Richard Bernstein, *Dictatorship of Virtue: Multiculturalism and the Battle for America’s Future* (New York, 1994), 48.

chive-exploring, note-taking, book-reviewing, manuscript-evaluating, article-and-book-writing, job-search-conducting, convention-attending, paper-presenting, critic-answering, professional-association-office-holding, and turf-defending. In the midst of the business and busy-ness of professional historians, the question of the purpose and goal of these activities—a question from which Turner did not try to hide—awaits our answer.

Native Americans, New Voices: American Indian History, 1895–1995

R. DAVID EDMUNDS

IN OCTOBER 1895, WHEN THE FIRST NUMBER of the *American Historical Review* was mailed to members of the American Historical Association, the United States was poised on the brink of a new century. The country had recently been troubled by economic depression, a growing army of populists, and serious labor unrest, and much of the American public worried that the nation was caught in a maelstrom of political, social, and economic dissent. Yet, as the decade continued, Americans grew more optimistic, confident that new political leadership and rapidly expanding technology would restore social order and economic prosperity. Indeed, as the Progressive Movement emerged, many intellectuals and academics subscribed to such optimism, confident that education, skillful management, and a renewed interest in the public's welfare would ensure political, economic, and social reform. The country had changed. Progress was inevitable. The nineteenth century was ending, and many vestiges of that century would either be abandoned or soon extinguished.

Most historians envisioned Native Americans and their history as part of this exclusion of what was past. In 1893, just two years before the initial publication of the *American Historical Review*, Frederick Jackson Turner delivered his now-famous essay at the annual meeting of the American Historical Association, in Chicago. The essay, which celebrates the frontier and the alleged march of American civilization across the continent, discusses the Native American role as both a facilitator and an opponent of such expansion. Turner argued that while "the Indian trade pioneered the way for civilization," the "Indian frontier" also served as a "consolidating agent . . . a common danger demanding united action." Yet, in 1893, both the frontier and Indian people seemed to be part of the past. Three years earlier, in 1890, the United States Bureau of the Census had reported that the frontier had vanished and that the Indian population had fallen to 248,253. Native Americans had played a major role in the history of the frontier, but the frontier was gone. For Turner and other historians, Indian people and their role in American history were also on the road to oblivion.¹

Several individuals made helpful suggestions in the preparation of this article. Their comments improved the essay, while the essay's faults are entirely the author's own. I would like to acknowledge the assistance of Michael McGerr, Wendy Gamber, Raymond DeMallie, Frederick Hoxie, Neal Salisbury, Michael Green, Theda Perdue, and Michael Leslie.

¹ Frederick Jackson Turner, "The Significance of the Frontier in American History," in George Rogers Taylor, ed., *The Turner Thesis: Concerning the Role of the Frontier in American History*, rev. edn.

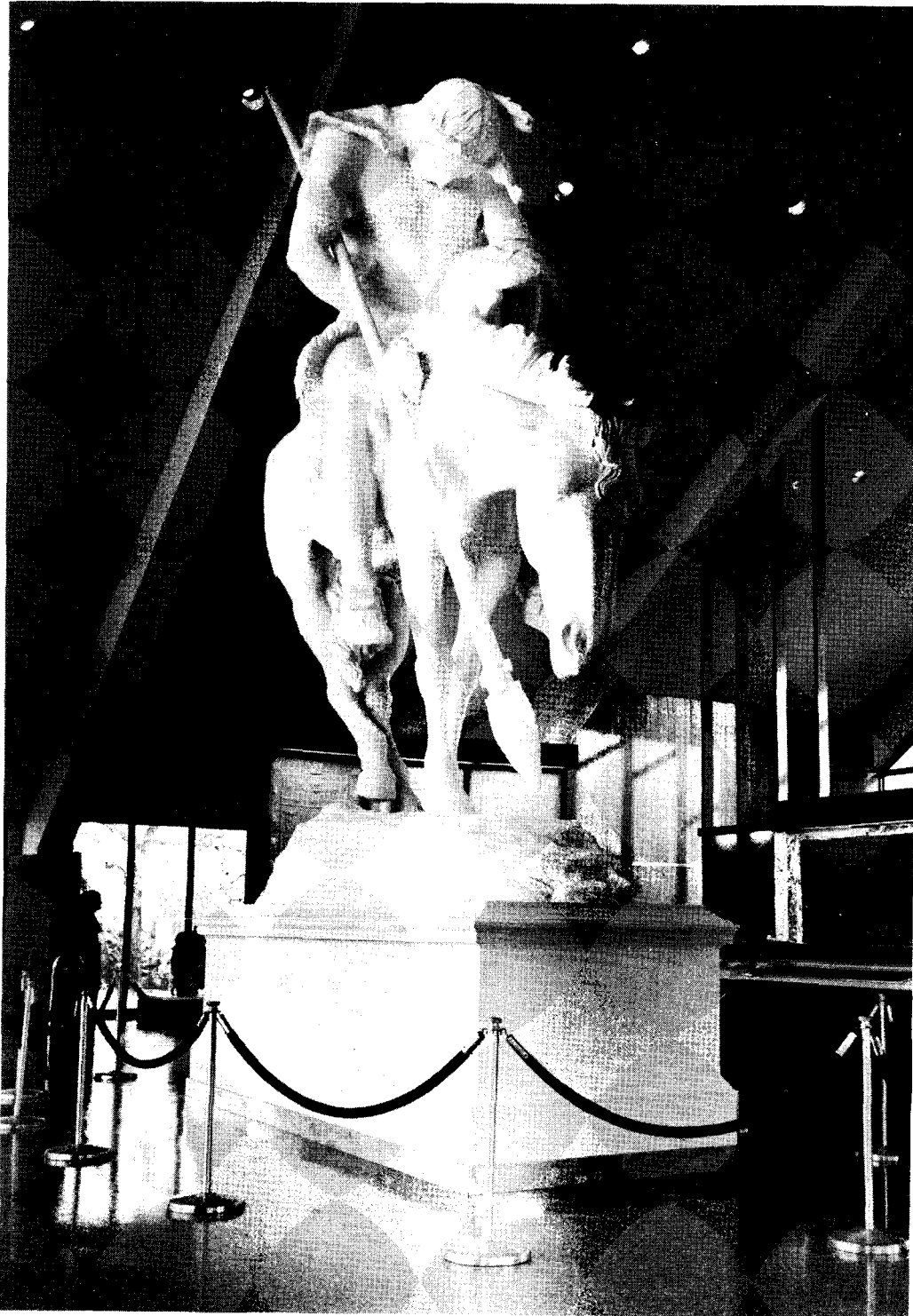
Other events in the 1890s seemed to confirm such predictions. In 1890, the year in which the frontier had officially “closed,” the Ghost Dance swept out of Nevada onto the Great Plains, where it found willing adherents among the Lakota. Confined to reservations and forced to walk the white man’s road, the Lakota welcomed the promise of a religious deliverance, but their acceptance of the new faith frightened Indian agents and ultimately led to the massacre at Wounded Knee, a “battle” that non-Indians envisioned as the last gasp of Native American resistance. Meanwhile, throughout the 1890s, federal agents busily implemented the Dawes Act, legislation designed to allot the reservations into small individual farms and to force Indian people into the American mainstream. Proponents of the act assured the American public that after the reservations were allotted, Indian people would accept their individual land holdings and would be completely assimilated. Native Americans, as a separate and unique ethnic minority group, would essentially disappear.²

Most Americans accepted this prognosis. Popular images of Indian people as romanticized “vanishing red men” permeated dime novels, popular magazines, and the newly emerging motion picture industry during this period, and almost all of these stereotypes focused on the past. Perhaps the classic manifestation of both the public’s and the intellectuals’ subscription to the “vanishing red man” concept can be found in James Earle Fraser’s popular sculpture, “The End of the Trail.” Fraser portrayed a defeated Plains Indian warrior mounted on a bedraggled pony; the man slumps forward, his head hanging down on his chest. Emphasizing Native Americans as part of a previous age, Fraser dressed his subject entirely in skin clothing and provided him with a stone-tipped lance. The trail slopes downward. Obviously, both the Indian and his culture are descending into oblivion. The 1900 census seemed to agree with Fraser’s portrayal. Returns from that year indicated that Indians numbered no more than 237,196, the historic nadir of Native American population.³

(Boston, 1956), 1–18; U.S. Bureau of the Census, *Report on Indians Taxed, and Indians Not Taxed in the United States (except Alaska)* (Washington, D.C., 1894), 24. The terms “Native American” and “Indian” are used interchangeably throughout this essay. Although “Native American” is the term of choice among academics, “Indian” is more commonly used among most of the Indian population in Oklahoma and on reservations in the West.

² For a good account of the Ghost Dance and its impact on the Lakota, see James Mooney, *The Ghost-Dance Religion and the Sioux Outbreak of 1890, Fourteenth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1892–1893, Part 2* (Washington, D.C., 1896); and Robert M. Utley, *The Last Days of the Sioux Nation* (New Haven, Conn., 1963). Frederick E. Hoxie, *A Final Promise: The Campaign to Assimilate the Indians, 1880–1920* (Lincoln, Neb., 1984), provides an excellent discussion of the Dawes Act and its failure to assimilate Native Americans into American society. Also see Janet A. McDonnell, *The Dispossession of the American Indian, 1887–1934* (Bloomington, Ind., 1991).

³ Alfred T. Collette and Donald M. Lantzy, *James Earle Fraser: The American Heritage in Sculpture* (Syracuse, N.Y., 1985). Preconceived images of Native Americans have long influenced both colonial and federal Indian policies. James Axtell points out that both Europeans and Native Americans first approached each other with preconceived notions regarding the other’s place in the scheme of things. Tragically, each side soon replaced early favorable assessments with more negative appraisals. Axtell argues that the new Indian assessments resulted from their exposure to European disease and aggrandizement, while Europeans used a more negative stereotype of Native Americans to facilitate their imperialism. See Axtell, “Imagining the Other: First Encounters in North America,” in Axtell, *Beyond 1492: Encounters in Colonial North America* (New York, 1992), 25–74. Also see Roy Harvey Pearce, *Savagism and Civilization: A Study of the Indian and the American Mind* (Baltimore, Md., 1967). Bernard W. Sheehan has argued that, despite Native American hospitality, colonists in Virginia were unable



Plaster version of the sculpture by James Earle Fraser (1876–1957). *The End of the Trail*, featured at the Panama-Pacific Exposition of 1915, now at the National Cowboy Hall of Fame and Western Heritage Center, Oklahoma City, Oklahoma. Our thanks to the National Cowboy Hall of Fame for supplying this photograph by Ed Muno.

THE PAGES OF THE *American Historical Review* reflected the same perspective. Although the "Documents" sections of the *AHR* included a few primary materials that focused on the Native American response to George Rogers Clark's campaign in the Illinois Country or discussed Indian relations as one of many factors affecting diplomacy during the Federalist period, the *AHR* published no essays on Native Americans during the first ten years of its existence.⁴ In July 1905, however, the journal published "The Indian Boundary Line," a ten-page essay by Max Farrand, a professor at Stanford University, which examined British Indian policy in the trans-Appalachian West between 1763 and 1776. In 1908, it was followed by E. G. Bourne's article "The Travels of Jonathan Carver," which investigated the authenticity of an early nineteenth-century memoir but used the ethnographic content of Carver's narrative to argue that his account was fictitious. Although a few other documents were published, no essay on Indians appeared for the next nine years. In October 1917, the *AHR* published Herbert E. Bolton's classic "The Mission as an Institution in the Spanish American Colonies" and two years later Verner Crane's "The Southern Frontier in Queen Anne's War," which examined British and French alliances among the Creeks, Choctaws, and Chickasaws.⁵

Almost all the documents and essays focusing on Native Americans that were printed in the *AHR* during the first quarter-century of its existence reflected the limited scope of historians' interest in Indian people in this period. With the exception of Bolton's essay, the articles featured Europeans and their institutions and discussed Native Americans primarily as objects of European or early American actions or policies. For example, Farrand's "Indian Boundary Line" examined the efforts of British Indian agents William Johnson and John Stuart to implement British policies in the West, but it failed to address how these policies resulted from earlier Native American efforts to retain control over their lands and economies. In addition, Indians were rarely portrayed as initiating any important activity; they participated in or responded to European initiatives but seemed to be incapable of formulating agendas of their own. They remained the supporting cast in a drama whose plot and leading roles were European.⁶

to transcend their belief in "ignoble savages" and that Jeffersonian Indian policy was an outgrowth of stereotyped images of Native Americans not based on reality. Sheehan, *Savagism and Civility: Indians and Englishmen in Colonial Virginia* (Cambridge, 1980); and Sheehan, *Seeds of Extinction: Jeffersonian Philanthropy and the American Indian* (Chapel Hill, N.C., 1973). Robert F. Berkhofer, Jr., *The White Man's Indian: Images of the American Indian from Columbus to the Present* (New York, 1978), provides an excellent survey of how changing images of Native Americans have influenced American science, literature, art, and Indian policy. Raymond William Stedman, *Shadows of the Indian: Stereotypes in American Culture* (Norman, Okla., 1982), focuses on Native American images in popular culture.

⁴ In Documents, see "Intercepted Letters and Journal of George Rogers Clark, 1778-1779," *AHR*, 1 (October 1895): 90-96; "Carondelet on the Defence of Louisiana, 1794," *AHR*, 2 (April 1897): 474-505; "The Illinois Indians to Captain Abner Prior, 1794," *AHR*, 4 (October 1898): 107-11; "English Policy toward America, 1790-1791, First Installment," *AHR*, 7 (July 1902): 706-35; "George Rogers Clark and the Kaskaskia Campaign, 1777-1778," *AHR*, 8 (April 1903): 491-507.

⁵ Max Farrand, "The Indian Boundary Line," *AHR*, 10 (July 1905): 782-91; E. G. Bourne, "The Travels of Jonathan Carver," *AHR*, 11 (January 1906): 287-302; Herbert E. Bolton, "The Mission as a Frontier Institution in the Spanish-American Colonies," *AHR*, 23 (October 1917): 42-61; Verner Crane, "The Southern Frontier in Queen Anne's War," *AHR*, 24 (April 1919): 379-95.

⁶ Farrand, "Indian Boundary Line," *passim*. Two books by Louise Phelps Kellogg epitomize this perspective. Until the 1960s, Kellogg's *The French Regime in Wisconsin and the Northwest* (Madison, Wis., 1925), and *The British Regime in Wisconsin and the Northwest* (Madison, 1935), served as the standard

Europeans also formulated the dialogue. Like other historians during this period, those scholars who wrote about events in which Native Americans played major roles based their research on traditional sources. Since government records, military reports, religious documents, and economic entries were usually generated by European males, accounts using these sources reflect their biases. Although women's historians have pointed out in the past three decades that male observers often failed to record the important contributions American women made in a broad spectrum of political and economic activities, or were ignorant of the distinct and separate "women's spheres" in which women spent their lives, at least these women shared a language and some cultural patterns with the male observers, and at least some American women left their own written (though often misinterpreted or ignored) records and memoirs. In comparison, Native Americans were culturally different from early literate white observers and did not share the same native language. Even though most tribes maintained a rich oral tradition, in the early twentieth century this cultural and historical information was often dismissed as "myth" or "legend" and rarely used by historians. Since Indians initially produced no written records of their own, accounts of their history were formulated by Europeans, using records or accounts written by other Europeans, many of whom had relatively limited familiarity with the Native American cultures and languages they were describing.

Tragically, one of the greatest blunders committed by historians peripherally interested in Indians at the turn of the century was their failure to collect or use the oral accounts held by many tribal members whose lifetime spanned much of the nineteenth century. Many of these individuals, or their parents or grandparents, had participated in events early in the nineteenth century. Since extended families and tribal communities continued to exist, these oral accounts could have provided a considerably enlarged Native American perspective. Unfortunately, during the twentieth century, much of this valuable information was lost.

The middle decades of the twentieth century brought few changes. Native Americans remained marginalized in American history, and many academic historians considered Native American history to be "popular history" or "cowboys and Indians," not worthy of serious research. In the four decades between 1920 and 1960, for instance, the *AHR* published only four articles on Native American subjects. Two written during the 1930s continued the pattern established at the beginning of the century. One examined American Indian policy in the Old Northwest during the War of 1812, while the second concentrated on British military policies and tactics during Braddock's Defeat, while generally ignoring the Indian forces responsible for inflicting the heaviest loss of life on a European or American army in all of American history. In 1949, a third essay examined John Evans's bizarre attempts to prove that prairie tribes such as the Omahas, Arikaras, and Mandans were of Welsh origin, but the essay focused on Welsh and American antiquarianism rather than the Indians. By far the most

histories of Indian-white relations in the western Great Lakes from the colonial period through the War of 1812. These are well documented, scholarly accounts, but they reflect the obvious European bias of their author.

perceptive article during these years was Mary Young's "Indian Removal and Land Allotment: The Civilized Tribes and Jacksonian Justice." Published in the October 1958 number, the essay discussed the implementation of Jacksonian Indian policy, but Young also described the Indian response and argued that intra-tribal politics contributed to the loss of allotments.⁷

Young's interest in Jacksonian Indian policy, the Five Southern Tribes, and the loss of Native American land reflected the focus brought to these subjects by two historians in Oklahoma. During the 1920s, Grant Foreman, an attorney initially employed by the Dawes Commission in Oklahoma, collected records documenting the history of the Five Southern Tribes, then wrote several volumes on the Indian experience in the removal period. Foreman's *Indian Removal*, published in 1932, provided the first comprehensive account of the removal of the Five Southern Tribes, while *Advancing the Frontier* examined their experiences upon arrival in the West. *The Last Trek of the Indians*, also published in 1932, provided brief summaries of the removal of many of the midwestern tribes. Although Foreman's work has been criticized as unsophisticated and biased in its sympathy toward the Indians, his volumes remain basic studies in this field. Moreover, in contrast to earlier studies, Foreman's writings concentrated on the experiences of Native Americans rather than white political or military figures.⁸

Angie Debo's publications followed a similar format. Trained as a professional historian (M.A., University of Chicago, 1924; Ph.D., Oklahoma University, 1933), Debo investigated the loss of Native American land through the misadministration of the Dawes Act. Her research indicated that prominent Oklahomans had participated in this fraud. Her life was threatened, and she was denied a teaching position in the state's universities; but volumes such as *Still the Waters Run* and *The Road to Disappearance* were highly acclaimed and brought her national recognition. Her studies of the Creeks and Choctaws during the latter half of the nineteenth century and her analysis of the subterfuge surrounding the loss of Indian land in Oklahoma are still the standard works on these subjects.⁹

Most Americans were more interested in Indian-white military confrontations. Historians such as George Hunt, Randolph Downes, Howard Peckham, and

⁷ Julius W. Pratt, "Fur Trade Strategy and the American Left Flank in the War of 1812," *AHR*, 40 (January 1935): 246–73; Stanley Pargellis, "Braddock's Defeat," *AHR*, 41 (January 1936): 253–69; David Williams, "John Evans' Strange Journey, Part II," *AHR*, 54 (April 1949): 508–29; Mary E. Young, "Indian Removal and Land Allotment: The Civilized Tribes and Jacksonian Justice," *AHR*, 64 (October 1958): 31–45.

⁸ William Welge, telephone interview by author, January 17, 1995. Welge is the curator of manuscripts at the Oklahoma Historical Society and is writing a biography of Foreman. See Grant Foreman, *The Five Civilized Tribes: Cherokee, Chickasaw, Choctaw, Creek, Seminole* (Norman, Okla., 1934); Foreman, *Indian Removal: The Emigration of the Five Civilized Tribes of Indians* (Norman, 1932); Foreman, *Advancing the Frontier, 1830–1860* (Norman, 1933); Foreman, *The Last Trek of the Indians* (Chicago, 1932). Since the Five Southern Tribes (Cherokees, Chickasaws, Choctaws, Creeks, and Seminoles) had adopted many Euro-American cultural patterns, non-Indians often referred to them as the "Five Civilized Tribes." I prefer the term "Five Southern Tribes," since all Native American people are "civilized" within the parameters of their own cultures.

⁹ Angie Debo, *The Rise and Fall of the Choctaw Republic* (Norman, Okla., 1934); Debo, *And Still the Waters Run* (Princeton, N.J., 1940); Debo, *The Road to Disappearance: A History of the Creek Confederacy* (Norman, 1941). Debo's life and career have been admirably portrayed in "Indians, Outlaws, and Angie Debo," produced for "The American Experience" series (PBS) by Barbara Abrash and Martha Sandlin, 1986.

Douglas Leach produced solid accounts of warfare east of the Mississippi, while other scholars shifted their focus westward to the encounters on the plains.¹⁰ At Oklahoma University, Walter S. Campbell, a professor of journalism who wrote under the pen name Stanley Vestal, collected personal accounts from warriors who had fought in the Sioux wars, and he published three volumes: *Sitting Bull: Champion of the Sioux*, *Warpath and Council Fire*, and *New Sources of Indian History*. Highly readable, the books make extensive use of Native American interviews and oral traditions.¹¹ Also written during this period, Mari Sandoz's *Crazy Horse: The Strange Man of the Oglalas* and *Cheyenne Autumn* focus on plains warfare and, like Vestal's accounts, incorporate considerable Native American testimony. The literary quality of Sandoz's works is exceptional, and they enjoyed both an academic and a broad general audience.¹²

Many of Foreman's, Debo's, and Vestal's books emerged from the University of Oklahoma Press; for, during the middle decades of the twentieth century, this press, under the directorship of Savoie Lottinville, formed the vanguard in the publication of Native American history. In 1932, the press published *Forgotten Frontiers*, the first of its now-famous "Civilization of the American Indian" series, which at present numbers over 215 volumes. The series includes biographies, volumes of edited documents, oral traditions, and ethnographic accounts but was noted for its publication of tribal histories, to which many prominent historians contributed studies. This tribal-history format has recently been criticized for its limited scope, yet many of these volumes remain the standard reference works on the individual tribes and serve as the basis for educational materials within the modern tribal communities.¹³

DURING THE 1960s, the study of Native American history was transformed. The emergence of the civil rights movement markedly increased both the public's and the academy's interest in the history of ethnic minority groups. As the consensus interpretation of American experience faded, many historians initially turned to

¹⁰ George T. Hunt, *The Wars of the Iroquois: A Study in Intertribal Relations* (Madison, Wis., 1940); Randolph C. Downes, *Council Fires on the Upper Ohio: A Narrative of Indian Affairs . . .* (Pittsburgh, Pa., 1940); Howard Peckham, *Pontiac and the Indian Uprising* (Princeton, N.J., 1947); Douglas Leach, *Flintlock and Tomahawk: New England in King Philip's War* (New York, 1958).

¹¹ Stanley Vestal, *Sitting Bull: Champion of the Sioux* (New York, 1932); Vestal, *Warpath and Council Fire: The Plains Indians' Struggle for Survival in War and Diplomacy, 1851–1891* (New York, 1948); and Vestal, *New Sources of Indian History, 1850–1891* (Norman, Okla., 1934). Also see Ray Tassin, *Stanley Vestal: Champion of the Old West* (Glendale, Calif., 1973).

¹² Mari Sandoz, *Crazy Horse: The Strange Man of the Oglalas* (New York, 1942); Sandoz, *Cheyenne Autumn* (New York, 1953).

¹³ Alfred Barnaby Thomas, ed., *Forgotten Frontiers: A Study of the Spanish Indian Policy of Don Juan Batista de Anza, Governor of New Mexico, 1777–1787* (Norman, Okla., 1932); John Drayton, telephone interview by author, February 2, 1995. Savoie Lottinville served as the editor of the press from 1938 through 1967. Examples of useful tribal histories from the series during this period include George E. Hyde, *Red Cloud's Folk: A History of the Oglala Sioux Indians* (1937); Hyde, *A Sioux Chronicle* (1956); William T. Hagan, *The Sac and Fox Indians* (1958); and John C. Ewers, *The Blackfeet: Raiders on the Northwestern Plains* (1958). The series continues to include excellent tribal histories. See Donald J. Berthrong, *The Southern Cheyennes* (1963), and *The Cheyenne and Arapaho Ordeal: Reservation and Agency Life in the Indian Territory, 1875–1907* (1976); Arrell M. Gibson, *The Chickasaws* (1971); W. David Baird, *The Quapaw Indians: A History of the Downstream People* (1980); and William Unrau, *The Kansa Indians: A History of the Wind People, 1673–1873* (1971).

the study of African Americans, but by 1968 scholars were also taking a new look at Native Americans and their contribution to the country's past. Unquestionably, the war in Vietnam added impetus to the latter inquiry. Historians who opposed the conflict drew similarities between interpretations of modern American imperialism in Southeast Asia and earlier American expansion onto Indian lands in the West. Meanwhile, the soldier's term "Indian Country," commonly applied to those regions of the Vietnamese countryside held by the Vietcong, reflected a broader, if less sophisticated, recognition of these parallels. In some instances, such similarities were overdrawn, but American uncertainty over involvement in the war gave credence to a newer, more critical evaluation of the government's relations with tribal people. It is not surprising that Dee Brown's *Bury My Heart at Wounded Knee*, while not very good history, struck a responsive chord with the American public during these years.¹⁴

The renewed interest in Native American history was also strengthened by the Red Power movement. Following the example of African Americans, younger, more militant Native American leaders appeared in the urban Indian communities and on university campuses in the West. While members of the American Indian Movement occupied Wounded Knee and the public spotlight, other activists in the urban Indian communities and on college campuses successfully petitioned university administrators to establish Native American Studies programs. Almost all these programs included courses in Native American history as part of their basic offerings, and history departments across the United States rushed to add undergraduate Native American history courses to their curricula. Although the job market was contracting, new openings in Native American history appeared. Eager for employment, many historians jumped on the buckskin bandwagon, marketed themselves as "Indian historians," and ventured forth into the classroom. Some were adequately trained; others were woefully lacking in their preparation.¹⁵

Ironically, the burgeoning interest in Native American history coincided with a significant change in methodology. Excellent studies of the formulation and administration of federal Indian policy continued, but scholars now attempted to develop a Native American perspective. For years, historians had complained that although anthropologists possessed a better understanding of tribal cultures, their historical research was inadequate, their prose was jargon ridden, and they often failed to place their analysis within a broader perspective. They knew what "was going on," but they did not know what "was happening." In rebuttal, anthropologists charged that historians were interested only in military or diplomatic affairs and were so dependent on written documents that they failed to understand the Native American viewpoint. They were writing "white man's history" about Native

¹⁴ Cecil Eby, *That Disgraceful Affair: The Black Hawk War* (New York, 1973), is a good example of an attempt to reinterpret Native American history through an obvious Vietnamese perspective. Also see Dee Brown, *Bury My Heart at Wounded Knee: An Indian History of the American West* (New York, 1970).

¹⁵ Native American Studies programs were established at the University of Minnesota in 1969, at UCLA in 1969, and at the University of California, Berkeley, in 1972. The first "American Indian" position to be advertised in the "Professional Register" section of the "AHA Newsletter" appeared in February 1969 (vol. 7, no. 3, p. 17): a position in "colonial and/or American Indian" at the University of Wisconsin, Stevens Point.

American people. They knew what “was happening,” but they really did not know what “was going on.”¹⁶

During the late 1950s, these conflicting perspectives converged (or at least hybridized) to form a new methodology called “ethnohistory.” For years, anthropologists such as Erminie Wheeler-Voegelin, John C. Ewers, and Anthony F. C. Wallace had pioneered this technique, but historians had been slow to adopt it, discounting the inclusion of a Native American viewpoint as speculative since such a perspective could not be documented by traditional means.¹⁷ In retrospect, however, Wallace’s anthropological study *The Death and Rebirth of the Seneca* probably marks a watershed, since it was widely acclaimed by both historians and anthropologists and served as a model for many younger historians (ethnohistorians) just beginning their careers. James Axtell has traced the growth of the discipline and argues that ethnohistory is “the use of historical and ethnological methods and materials to gain knowledge of the nature and causes of change in a culture defined by ethnological concepts and categories.” Some of its adherents still disagree among themselves over the proper definition of the term, but during the past two decades its methodology has been widely accepted.¹⁸

The “new Indian history” employs this methodology of ethnohistory. Designed to place the tribal communities within the broader American perspective, this history also illustrates how Native American people were motivated by their unique cultural patterns and how those patterns adapted to change. Although Indian people have been forced to react to European and American policies, the new Indian history attempts to analyze the Native American response and to demonstrate that tribal cultures have been remarkably resilient, maintaining many of their traditions through decades of forced acculturation. In addition, this new scholarship has endeavored to present an “Indian-centered” perspective: an account of Indian-white relations that analyzes this interaction from the Native American point of view. Indian people are no longer portrayed solely as pawns of

¹⁶ Foremost among those scholars who have continued to produce excellent studies of American Indian policy is Francis Paul Prucha. In a career that has spanned four decades, Prucha has written or edited numerous volumes and essays on the development and administration of federal policy. His volumes are too numerous to mention, but perhaps the capstone of his scholarship is *The Great Father: The United States Government and the American Indians*, 2 vols. (Lincoln, Neb., 1984), a masterful survey of the subject. Even historians who disagree with some of his interpretations admire and rely on his meticulous scholarship.

¹⁷ The American Society for Ethnohistory emerged from the efforts of Erminie Wheeler-Voegelin, an anthropologist at Indiana University. Employed by the Indian Claims Commission, during the 1950s Wheeler-Voegelin directed a project that amassed considerable quantities of documents and early secondary materials focusing on residency and land use by the tribes of the Great Lakes and northern Ohio Valley. In 1953, scholars interested in this subject attended a conference at Ohio State University and formed the Ohio Valley Historic Indian Conference, which evolved into the American Society for Ethnohistory. *Ethnohistory*, the journal of the society, began publication in 1954.

¹⁸ See John C. Ewers, *The Horse in Blackfoot Indian Culture, with Comparative Material from Other Western Tribes* (Washington, D.C., 1955); and Ewers, *Blackfeet*. Also see Anthony F. C. Wallace, *The Death and Rebirth of the Seneca* (New York, 1970). Also see Calvin Martin, “Ethnohistory: A Better Way to Write Indian History,” *Western History Quarterly*, 9 (January 1978): 41–56; and James Axtell, “Ethnohistory: An Historian’s Viewpoint,” in Axtell, *The European and the Indian: Essays in the Ethnohistory of Colonial North America* (New York, 1981), 5. Axtell’s definition is probably the most widely accepted general definition of “ethnohistory,” but in 1989 I interviewed numerous anthropologists and historians and found that there still was considerable disagreement. One historian, who admitted that he could not precisely define the discipline, stated, “Whatever it is, John Ewers does it.”

federal policy; they developed their own methods of manipulating a system designed to control them.

The emergence of the new Indian history is closely associated with the D'Arcy McNickle Center for the History of the American Indian, at the Newberry Library in Chicago. Founded in 1972, the center has hosted dozens of workshops and conferences and has awarded hundreds of fellowships to academic historians, public school teachers, and tribal historians, who have traveled to the Newberry to use its rich resources. The academic historians in the center's fellowship program currently include most of the world's leading scholars in this field, and many of the secondary teachers and tribal historians who have participated in the workshops have used their residency in Chicago to develop new instructional materials for use in their classrooms and tribal communities. Unquestionably, the McNickle Center has played a critical role in the growth and development of the new Indian history. As one historian has noted, "the McNickle Center has become for Indian history what Paris is for fashions."¹⁹

Since 1970, the new Indian history has expanded in many directions. No longer interested primarily in federal policy or military affairs, historians have extended their investigations to subjects or periods previously ignored. During the past decade, the pre-Columbian period has attracted considerable attention. Long dismissed as irrelevant to the "mainstream" of American history, pre-Columbian Native Americans had been dehumanized in opening sections of textbooks, which often included pre-Columbian societies in general discussions of climate, topography, flora, and fauna. Unfortunately, most data on pre-Columbian societies had been supplied by archaeologists, who were more interested in describing and classifying artifacts than in discussing the people and societies who used them. Consequently, as Samuel Eliot Morrison stated in *The Oxford History of the American People*, "when we try to tell the story of man in America . . . , the lack of data brings us to a halt. There are plenty of surviving objects . . . but no written records . . . [T]he history of the American People is the history in America of immigrants from other countries." Surveys of American history textbooks conducted as late as 1986 indicated that many authors and publishers still subscribed to such reasoning.²⁰

More recent scholarship has altered this pattern. During the past decade, some authors of American history survey textbooks have reexamined the pre-Columbian past and have placed pre-Columbian cultures within a broader context. Instead of depicting pre-Columbian North America as a cultural backwater, isolated from the

¹⁹ Frederick Hoxie, telephone interview by author, January 25, 1995.

²⁰ Samuel Eliot Morrison, *The Oxford History of the American People* (New York, 1965), 3. Morrison does include a preliminary chapter of fifteen pages, "America under Her Native Sons," at the beginning of his survey. The entire volume, excluding the index, is some 1,122 pages. Recently, some archaeologists have adopted new methods of interpreting and discussing the pre-Columbian past. A good example of a relatively jargon-free, humanistic approach to the use of archaeological materials is in Janet D. Spector, *What This Awl Means: Feminist Archaeology at a Wahpeton Dakota Village* (St. Paul, Minn., 1993). An interesting attempt to combine archaeological evidence with tribal oral traditions can be found in Roger Echo-Hawk (Pawnee), "Kara Katit Pakutu: Exploring the Origins of Native America in Anthropology and Oral Traditions" (M.A. thesis, University of Colorado, 1994). James Axtell's survey, "Europeans, Indians, and the Age of Discovery in American Indian Textbooks," *AHR*, 92 (June 1987): 621-32, obviously focuses on the "Age of Discovery" but also indicates that textual coverage of both the pre-Columbian and subsequent periods is inadequate and often error ridden.

general development of “civilized” societies in the Old World, historians have placed pre-Columbian cultures into a world-wide pattern, with manifestations similar to those of contemporary societies in Africa and Eurasia. Focusing on the Adena, Hopewellian, and Mississippian people of the eastern United States, scholars have drawn parallels to contemporary cultures in Europe or Mesopotamia. For example, the Mississippians, a riverine people, flourished between 700 and 1700 A.D. and constructed large earthen mounds that served as ceremonial and population centers. Governed by a theocracy, Cahokia, a Mississippian community opposite modern St. Louis, emerged as a nascent city-state that dominated the surrounding river valleys. An important trade center, in 1100 A.D. Cahokia had a population exceeding ten thousand. Several American history textbooks published within the past decade have featured Cahokia as indicative of the pre-Columbian societies, and interest in the pre-Columbian period continues. In 1992, *America in 1492: The World of the Indian Peoples before the Arrival of Columbus*, a volume edited by Alvin M. Josephy, Jr., and sponsored by the McNickle Center, offered a series of essays surveying pre-Columbian cultures; and, more recently, other scholars such as Charles Hudson and Francis Jennings have attempted to investigate the connections between pre-Columbian societies and historic tribes in the United States.²¹

Critical to the renewed interest in pre-Columbian societies are the revised estimates of pre-Columbian populations. In 1965, Morrison’s *Oxford History of the American People* indicated that, although “the population of the Americas in 1500 is largely a matter of conjecture . . . , the latest scholarly estimates of Indian population in the present area of Canada and the United States vary from 900,000 to 1,500,000.” Today, these estimates have increased almost fivefold. Current figures range from 5 million to 12.5 million, with perhaps 6 to 7 million emerging as the most commonly accepted estimate. Obviously, the increased numbers give credence to the arguments for more sophisticated pre-Columbian societies, since larger populations could more easily provide the goods and services needed to support a complex, stratified society. Moreover, like “civilizations” in Eurasia, the pre-Columbian population and its complex societies appeared to rise and fall. Cahokia reached the height of its population and influence in about 1250 A.D.; by 1400, it had been abandoned.²²

Yet other, more ominous population losses loomed on the horizon, and recent

²¹ Some American history textbooks now include a chapter or an enlarged section on pre-Columbian America. See John Mack Faragher, Mari Jo Buhle, Daniel Czitrom, and Susan H. Armitage, *Out of Many: A History of the American People* (Englewood Cliffs, N.J., 1994), vol. 1; Paul S. Boyer, Clifford E. Clark, Sr., Joseph F. Kett, Neal Salisbury, Harvard Sitkoff, and Nancy Woloch, *The Enduring Vision: A History of the American People* (Lexington, Mass., 1993), vol. 1; and Gary B. Nash, Julie Roy Jeffrey, John R. Howe, Peter J. Frederick, Allen F. Davis, and Allan M. Winkler, *The American People: Creating a Nation and a Society* (New York, 1987), vol. 1. Also see Alvin M. Josephy, Jr., *America in 1492: The World of the Indian Peoples before the Arrival of Columbus* (New York, 1992); Charles Hudson and Carmen Chaves Tesser, eds., *The Forgotten Centuries: Indians and Europeans in the American South, 1521–1704* (Athens, Ga., 1994); and Francis Jennings, *The Founders of America: How Indians Discovered the Land, Pioneered in It, and Created Great Classical Civilizations, How They Were Plunged into a Dark Age by Invasion and Conquest, and How They Are Reviving* (New York, 1993).

²² Morrison, *Oxford History of the American People*, 15. For more recent discussions of the Native American population north of Mexico in 1492, see Henry F. Dobyns, *Their Number Become Thinned: Native American Population Dynamics in Eastern North America* (Knoxville, Tenn., 1973); Russell Thornton, *American Indian Holocaust and Survival: A Population History since 1492* (Norman, Okla.,

scholarship has analyzed their magnitude. In 1540, when Hernando De Soto journeyed across the Southeast, he encountered populous Mississippian societies residing in fortified towns surrounded by extensive fields of corn and other crops. De Soto was received by powerful leaders, draped with pearls, who rode in sedan chairs; when hostile warriors opposed his entrance, they fought in well-organized ranks, commanded by native officers. One century later, these populous societies were gone, swept away by a pestilential holocaust. Within the past two decades, scholars such as Alfred W. Crosby, Henry Dobyns, and Russell Thornton have investigated these events and have demonstrated that because Native American peoples possessed no natural immunities to Old World pathogens, they succumbed to these epidemics by the millions. At first, such a catastrophic loss of human life seemed almost incomprehensible, and some historians were reluctant to accept the magnitude of these figures; but, when demographers examined the impact of smallpox and other diseases on historic tribal communities, they could confirm by extrapolation that the initial estimates were valid. Indeed, scholars such as Peter Wood have shown that the epidemics continued to ravage the Mississippian homeland well into the eighteenth century.²³

The implications of such a holocaust are profound. In the early seventeenth century, when the British and French first established settlements on the eastern seaboard, they did not encounter well-organized, populous incipient city-states. Instead, they met the scattered remnants of a Native American population devastated by disease. The pandemics were so disruptive that they destroyed the social and political structures of most Mississippian peoples and played havoc with simpler woodland societies along the northeast coast. The golden age of the Mississippians was gone. The towns and ceremonial centers had been abandoned. Native Americans had been plunged into a dark age not of their own making. They were vulnerable to the European immigrants who landed on the edges of their world.²⁴

NATIVE AMERICANS WERE OVERWHELMED, but they also persisted. Unlike historians of the early twentieth century, who portrayed Indian people as “vanishing Americans” on “the road to disappearance,” recent scholars have emphasized

1987); and Josephy, *America in 1492*, 6. Also see Thomas E. Emerson and R. Barry Lewis, eds., *Cahokia and the Hinterlands: Middle Mississippian Cultures of the Midwest* (Urbana, Ill., 1991).

²³ Thornton, *American Indian Holocaust*; Dobyns, *Their Number Become Thinned*. See Alfred W. Crosby, Jr., *The Columbian Exchange: Biological and Cultural Consequences of 1492* (Westport, Conn., 1971). Crosby presents an excellent discussion of the biological impact of the “discovery” of the Americas on both eastern and western hemispheres. Also see Peter Wood, “The Changing Population of the Colonial South: An Overview by Race and Region, 1685–1790,” in Peter H. Wood, Gregory A. Waselkov, and M. Thomas Hatley, eds., *Powhatan's Mantle: Indians in the Colonial Southeast* (Lincoln, Neb., 1989), 35–103.

²⁴ See Jennings, *Founders of America*; and Francis Jennings, *The Invasion of America: Indians, Colonialism, and the Cant of Conquest* (Chapel Hill, N.C., 1975). David E. Stannard, *American Holocaust: Columbus and the Conquest of the New World* (New York, 1992), presents an even more critical analysis of the European conquest. By 1680, only one viable Mississippian society, the Natchez, remained. Led by the “Great Sun,” a hereditary leader with almost absolute power, the Natchez were attacked and destroyed by the French during the 1720s.

Native American adaptability and perseverance. Indeed, it is within the realm of the seventeenth and eighteenth centuries, the first period of sustained contact between Native Americans and European settlers in the United States, that the new Indian history has made the most profound impact. Since 1982, although studies by William Cronon, Neal Salisbury, Richard White, and Daniel Usner have clearly demonstrated that Indian societies used land, resources, and commodities in different ways from Europeans, they also indicate that Native Americans adapted European technology and economic patterns to their own needs.²⁵ Other historians have focused on the Iroquois Confederacy, tracing these nations' emergence and role as political power-brokers during the colonial period. Recently, claims by some Native American historians that the Constitution of the United States was modeled after the Iroquois confederacy have attracted the public's attention and engendered considerable controversy.²⁶

Indians also played key roles in the formation of new American societies. Even though Native American people were rarely integrated into the British colonies that clustered along the eastern seaboard, Richard White has argued that they joined with French traders in the interior to form a cultural and genetic "middle ground," a way of life in which Indian and French worlds "melted at the edges and merged," and in which it became unclear "whether a particular practice or way of doing things was French or Indian."²⁷ Until the 1970s, these multi-ethnic cultures of the American interior had received scant attention. Dismissed as marginal societies, caught between the Indian and European worlds, they were once depicted as tenuous, both in population and duration. More recently, scholars have reappraised these societies and have established that many were dominated by people of mixed lineage, who, far from being outcasts between two cultures, bridged cultural gaps between these groups and also served as intermediaries between frontier societies and European or American governments. In the Great Lakes region, métis, or mixed-blood, leaders such as Charles Langlade (Ottawa), Siggenauk (Potawatomi), and Jean Baptiste Ducoigne (Kaskaskia) rallied both

²⁵ William Cronon, *Changes in the Land: Indians, Colonists, and the Ecology of New England* (New York, 1983); Neal Salisbury, *Manitou and Providence: Indians, Europeans, and the Making of New England, 1500–1643* (New York, 1982); Richard White, *The Roots of Dependency: Subsistence, Environment, and Social Change among the Choctaws, Pawnees, and Navajos* (Lincoln, Neb., 1983); Daniel H. Usner, Jr., *Indians, Settlers, and Slaves in a Frontier Exchange Economy: The Lower Mississippi Valley before 1783* (Chapel Hill, N.C., 1992). Albert L. Hurtado has shown that Native Americans in California also were adaptive but that they were overwhelmed by demands for their land and labor after the arrival of Anglo-Americans during the late nineteenth century. See Hurtado, *Indian Survival on the California Frontier* (New Haven, Conn., 1988).

²⁶ Daniel K. Richter, *The Ordeal of the Longhouse: The Peoples of the Iroquois League in the Era of European Colonization* (Chapel Hill, N.C., 1992); Francis Jennings, *The Ambiguous Iroquois Empire: The Covenant Chain Confederation of Indian Tribes with English Colonies from Its Beginnings to the Lancaster Treaty of 1744* (New York, 1984); Daniel K. Richter and James H. Merrell, eds., *Beyond the Covenant Chain: The Iroquois and Their Neighbors in Indian North America, 1600–1800* (Syracuse, N.Y., 1987). In *Exemplar of Liberty: Native America and the Evolution of Democracy* (Los Angeles, 1991), Donald A. Grinde, Jr., and Bruce E. Johansen argued that the political theories of the "founding fathers" were heavily influenced by their familiarity with the political structure of the Iroquois Confederacy. Their assertions have created considerable debate and have attracted both the media, the public, and Congress. Also see Oren Lyons (Onondaga), John Mohawk (Seneca), et al., *Exiled in the Land of the Free: Democracy, Indian Nations, and the U.S. Constitution* (Santa Fe, N.Mex., 1992).

²⁷ Richard White, *The Middle Ground: Indians, Empires, and Republics in the Great Lakes Region, 1650–1815* (Cambridge, 1991), 50.

natives and creole French in support of the French, British, or American cause. These leaders enjoyed the support of the Indian communities, but according to George Rogers Clark, many conducted themselves "as much in the French manner as possible."²⁸

South of the Ohio, men such as Alexander McGillivray (Creek) and William McIntosh (Creek) combined Native American and Scots or Irish ancestries to emerge as influential figures. They led multi-ethnic societies of Indian, European, and African-American members along a diplomatic tightrope between Spanish, British, and American governments. Moreover, scholars James H. Merrell, Richard White, Michael Green, and Daniel Usner have shown that, for many Indians in the South, the European and African entrance created an "Indians' New World," which, in many ways, was as "new" for Native Americans as it was for the Europeans. Although the multi-ethnic component of these societies differed markedly among different tribal communities, studies of these societies have provided insights into the development of southern attitudes toward slavery and race.²⁹

The evolution of Indian leadership in the early decades of the nineteenth century also has undergone considerable revision. New analyses of Native American resistance movements prior to the War of 1812 have de-emphasized the role of Indians as mere allies of the British and have pointed out that leaders such as Tecumseh, who attempted to forge a pan-Indian political and military alliance, championed agendas very much their own. Moreover, Gregory Dowd, Joel Martin, and other scholars have shown that religious revitalization movements markedly influenced this resistance but that many Indian people were alienated by such nativism and supported the United States. Both historians and the general public have long been fascinated with Tecumseh, and recent scholarship has separated the man from the myth and analyzed his appeal to non-Indians.³⁰

Most mixed-bloods opposed Tecumseh's efforts, and in the decades following the War of 1812, their influence increased. Historians such as Foreman or Debo earlier chronicled the rise of an acculturated mixed-blood population among the southern tribes, but since 1970 scholars have analyzed the methods used by

²⁸ George Rogers Clark to John Brown, 1791, *George Rogers Clark Papers*, James Alton James, ed., *Collections of the Illinois State Historical Library*, 38 vols. (Springfield, Ill., 1903-78), 8: 252-55. The nomenclature of bicultural people of mixed lineage continues to cause problems. "Mixed-blood" currently is the most widely used term, but some academics dislike it because of its emphasis on race, not culture. People of French-Indian descent have long been known as "métis" in Canada and the Great Lakes region, and today "métis" is often used to refer to people of mixed Indian and European lineage in that region. Recently, some scholars have suggested that "bicultural" should be used, particularly in reference to people of mixed lineage and culture among the southern tribes. Gerald Vizenor (Anishinaabe) has used terms such as "crossblood" or "postindian" in his novels of modern Native American life. Since "mixed-blood" is the most common term employed within the Native American communities, it will be used in this essay.

²⁹ James H. Merrell, *The Indians' New World: Catawbas and Their Neighbors from European Contact through the Era of Removal* (Chapel Hill, N.C., 1989); Richard White, *The Roots of Dependency: Subsistence, Environment, and Social Change among the Choctaws, Pawnees, and Navajos* (Lincoln, Neb., 1983); Michael D. Green, *The Politics of Indian Removal: Creek Government and Society in Crisis* (Lincoln, 1982); J. Leitch Wright, Jr., *Creeks and Seminoles: The Destruction and Regeneration of the Muscogulge People* (Lincoln, 1986); Usner, *Indians, Settlers, and Slaves in a Frontier Exchange Economy*.

³⁰ Gregory Evans Dowd, *A Spirited Resistance: The North American Indian Struggle for Unity, 1745-1815* (Baltimore, Md., 1992); Joel W. Martin, *Sacred Revolt: The Muskogees' Struggle for a New World* (Boston, 1991). Also see R. David Edmunds, *The Shawnee Prophet* (Lincoln, Neb., 1983); and Edmunds, *Tecumseh and the Quest for Indian Leadership* (Boston, 1984).

mixed-bloods to attain their positions of leadership and have also examined how they attempted to change tribal societies. The late William McLoughlin argued that many traditional, conservative Cherokees transformed the teachings of Protestant missionaries into a syncretic faith, combining traditional tribal religious beliefs with the Christian doctrines. In contrast, other scholars have argued that missionaries played a key role in providing the mixed-bloods with both the formal education and the moral support to enable them to centralize their tribes' political structures. Unlike earlier historians, modern scholars have focused on intra-tribal politics, tracing changes from older, more traditional patterns to the statutory structures of the late 1820s and 1830s. Championing the concept of private property, the mixed-bloods defended the retention of tribal homelands since they had developed plantations, farms, or other enterprises in the region. In turn, their defense of the tribal estate attracted the support of the less acculturated members of their tribes and gave their centralization of power a veneer of legitimacy. Yet, like the opportunistic American society that surrounded them, many of these mixed-blood leaders used their position to enhance their own fortunes. In reality, they were far removed from the common stereotypes of Indian leaders as war chiefs, "noble savages," or victims. As the new Indian history has illustrated, they were complex figures, representatives of complex societies, but they were people who controlled their own fortunes and who did much to shape the wealth of their tribal communities.³¹

Much less attention has been given to the Native American societies north of the Ohio. White's *Middle Ground* traces the development of these societies through the War of 1812, but relatively few scholars have examined their subsequent history, and most of these studies have been limited by their rather traditional, monographic approach. Initial investigations indicate that a métis leadership among tribes such as the Potawatomis and Miamis provided role models for less acculturated members of these tribes and that métis entrepreneurs played a major role in the Indian trade in much of the region during this period. Many of these métis merchants were relatively well-educated, prosperous individuals. (In 1816, when Indiana entered the union, Jean Baptiste Richardville, a Miami trader, was reputed to be the wealthiest man in the state.) Because they refused to engage in agriculture and occupied good farm land, however, they were condemned as "savages" and removed west of the Mississippi.³²

Transplanted Native American societies temporarily flourished in the West. In Indian Territory, the Five Southern Tribes rebuilt political and socioeconomic

³¹ Between 1984 and 1990, William G. McLoughlin published four volumes that analyze and discuss Cherokee society during the first four decades of the nineteenth century: *Cherokees and Missionaries, 1789–1839* (New Haven, Conn., 1984); *The Cherokee Ghost Dance: Essays on the Southeastern Indians, 1789–1861* (New Haven, 1986); *Cherokee Renaissance in the New Republic* (New Haven, 1986); and *Champions of the Cherokees: Evan and John B. Jones* (Princeton, N.J., 1993). Also see White, *Roots of Dependency*; Green, *Politics of Indian Removal*; and Theda Perdue, *Slavery and the Evolution of Cherokee Society, 1540–1866* (Knoxville, Tenn., 1979).

³² White, *Middle Ground*. Also see Gary C. Anderson, *Kinsmen of Another Kind: Dakota-White Relations in the Upper Mississippi Valley, 1650–1862* (Lincoln, Neb., 1984); R. David Edmunds, *The Potawatomis: Keepers of the Fire* (Norman, Okla., 1978); Stewart Rafert, *The Miami Indians of Indiana, 1654–1994: People of the Middle Ground* (Indianapolis, Ind., forthcoming, 1996); Sarah E. Cooke and Rachel Ramadhyani, eds., *Indians and a Changing Frontier: The Art of George Winter* (Bloomington, Ind., 1993).

structures that had emerged prior to their removal, and, although these societies were plagued by the bitter vestiges of removal politics, many of their members prospered. Tribal membership provided access to economic opportunities, and in some instances tribal societies formed a socio-cultural elite, growing numbers of whites married into the tribes, and the pace of acculturation accelerated. Meanwhile, in Kansas, many of the Potawatomi métis flourished as merchants, providing merchandise and services to emigrants en route to the gold fields in Colorado. Tragically, all of these transplanted Indian societies were decimated by the Civil War, but for two decades preceding the conflict they served as focal points for the introduction of new cultural patterns into the trans-Mississippi west. Ironically, like their white counterparts, they, too, served as "pioneers." Although scholars have begun to examine this process, the character and impact of these transplanted societies need more investigation.³³

While the transplanted societies developed in the West, remnant groups from these communities remained behind, forming the nucleus of a surprisingly large Native American population, which has continued to grow throughout the twentieth century. Significant numbers of Cherokees and Choctaws stayed in North Carolina and Mississippi, while much smaller communities of Native Americans were still in place from the Great Lakes to the Northeast. Some continued to occupy reservations (Eastern Cherokees, Ottawas, Menominees, for instance), but others resided on private lands, and federal officials refused to recognize them as Indians. Within the past two decades, many of these smaller, more diffuse Native American communities have petitioned the federal government for recognition but with mixed success. In 1992, for example, the Bureau of Indian Affairs refused to recognize the Miamis of Indiana but two years later extended federal recognition to the Pokagon Potawatomis, a community in southern Michigan. Why have these smaller, mixed-blood communities, though relatively acculturated, clung to their Indian identities? Devoid of federal recognition, what strategies have they adopted, conscious or otherwise, to maintain their sense of cohesion? Late in the twentieth century, as changing legal interpretations of tribal sovereignty offer Indian communities new economic opportunities (tax-free enterprise zones and gaming), some of these formerly marginalized communities have emerged as economic juggernauts. The historic antecedents of these modern entrepreneurs and their new economic activities offer ample opportunity for future scholarship, as does the impact of such individuals and communities on shifting definitions of Native American identity.³⁴

³³ Debo examined these societies during the second quarter of the twentieth century, but her analysis was limited and lacked an ethnohistorical perspective. See Debo, *Rise and Fall of the Choctaw Republic; And Still the Waters Run; and Road to Disappearance*. More recently, William G. McLoughlin, *After the Trail of Tears: The Cherokees' Struggle for Sovereignty, 1839–1880* (Chapel Hill, N.C., 1993), examines Cherokee politics in the post-removal period, while Duane Champagne, *Social Order and Political Change: Constitutional Governments among the Cherokee, the Choctaw, the Chickasaw, and the Creek* (Stanford, Calif., 1992), also contains chapters on the reconstruction of Indian political structures in the West. Also see H. Craig Miner, *The Corporation and the Indian: Tribal Sovereignty and Industrial Civilization in Indian Territory, 1865–1907* (Columbia, Mo., 1976); and Daniel F. Littlefield, Jr., *The Chickasaw Freedmen: A People without a Country* (Westport, Conn., 1980). Littlefield has written extensively on the interaction of Indians, African Americans, and whites in Indian Territory.

³⁴ John R. Finger, *The Eastern Band of Cherokees, 1819–1900* (Knoxville, Tenn., 1984); and *Cherokee Americans: The Eastern Band of Cherokees in the Twentieth Century* (Lincoln, Neb., 1991), provide a good

AT THE END of the twentieth century, questions of Native American identity continue to plague both Indian history and Indian politics. Obviously, during much of the early colonial period, issues of identity were almost nonexistent, since everyone in a tribal community knew everyone else or at least was familiar with an individual's kinship affiliations. Moreover, individual Native Americans envisioned themselves as members of an extended kinship network, and group identity was focused first on clan, then on a much more ambiguous tribal affiliation. In other words, one saw oneself first as a member of the Fox clan, then as a Mesquakie. If captives or other outsiders were adopted into a family, they became part of the clan and consequently were part of the "tribal community." Under such conditions, tribal membership was communally self-determined. Regardless of one's origin or ethnicity, if the Shawnees said you were Shawnee, you *were* Shawnee.

After half a century of contact, the parameters of identity began to change. Confronted by Europeans who classified all tribal peoples as "Indians," some Native Americans, while retaining their clan and tribal identities, began to identify themselves in opposition to the Europeans: as members of an indigenous American ethnic group whose broader membership transcended clan or tribal boundaries. As European or American expansion crossed the Appalachians, more tribespeople subscribed to the broader definition, and Native American leaders such as Pontiac or Tecumseh unsuccessfully attempted to channel such identification into pan-tribal political movements.³⁵

Their efforts failed, and by the 1830s Indian communities were pressured to accept arbitrary and very selective foreign definitions of Native American and even tribal identity. At issue were the mixed-bloods. Although people of mixed lineage generally were accepted as Native Americans by their tribal communities, they were highly suspect to federal officials, who often based their definition of ethnicity on an individual's willingness to cooperate with the government. If mixed-blood leaders acquiesced in federal demands for land cessions, they were described as legitimate "chiefs and spokesmen" for the tribal communities, but if they opposed federal policy they were denounced as "degraded white men."³⁶

It is the definitions of identity imposed by the Dawes Commission that have caused problems in the twentieth century. During the 1890s, when federal officials allotted the reservations, they refused to accept tribally defined membership,

survey and analysis of this group, while Frank W. Porter III, ed., *Strategies for Survival: American Indians in the Eastern United States* (Westport, Conn., 1986), focuses on small communities along the central and northeastern Atlantic coast. Also see James Clifton, *The Pokagons, 1683–1983: Catholic Potawatomi Indians of the St. Joseph River Valley* (Lanham, Md., 1983); and Rafert, *Miami Indians*.

³⁵ Almost all scholars agree that, after a relatively short period, Native Americans from different tribal communities began to envision themselves as a distinct and common identity (Indians) in opposition to "white men" or Europeans, but historians disagree over the parameters and especially the political cohesion of such an identity. See White, *Middle Ground*; Dowd, *Spirited Resistance*; Edmunds, *Tecumseh and the Quest for Indian Leadership*; and Michael N. McConnell, *A Country Between: The Upper Ohio Valley and Its People, 1724–1774* (Lincoln, Neb., 1992).

³⁶ "Journal of the Proceedings Held on the St. Joseph's of Lake Michigan, September, 1828," Ratified Indian Treaties, Record Group 11, National Archives (M668, Roll 6, 192–196); George W. Ewing to the Secretary of War, February 12, 1837, Office of Indian Affairs, Letters Received, Record Group 75, National Archives (M234, Roll 355, 824–828).

demanding that Native Americans enroll with federal allotment agents and state their "blood quantum," or degree of Indian ancestry. Although most tribe members had not previously considered blood quantum to be a primary factor in defining their constituency, during the twentieth century many adopted this government dictum as a regulatory mechanism in their own definition of tribal membership. For some tribes, this measure has had little effect, but for others it has served as an exclusionary device, debarring individuals who are only culturally Indian.³⁷ Meanwhile, since 1970, the self-identified (U.S. Census) Native American population in the United States has tripled, as growing numbers of Americans with at least partial Indian ancestry have attempted to renew or reclaim their Native American identity. Now, as in the past, Native American identity remains ambiguous. Historians need to investigate the circumstances that have periodically prompted non-Indian Americans to join or identify with the Native American communities. "Being Indian" has meant different things at different times, but the changing nature of Native American identity offers ample opportunity for future scholarship.³⁸

Native American identity has been strongly influenced by the enrollment of Indian children in non-Indian schools. Since the colonial period, Native American parents have been either encouraged or coerced to enroll their children in educational institutions over which they exercised minimal control. The experience has markedly affected Native American concepts of self-identity and has also engendered divisions within the tribal communities over the role of their members in American society. Educational institutions have consistently functioned as agents of acculturation, and during the nineteenth and early twentieth centuries their curricula and Dickensian methods of instruction often reflected a callous insensitivity to Native American children. Students were encouraged to renounce their traditional culture and to plunge headfirst into the mainstream of American life. Only a complete rejection of tribal identities would assure their assimilation into modern American society. Yet this promise failed. The adherence to white cultural patterns did not guarantee acceptance by non-Indians, and cultural patterns acquired at boarding schools often attracted criticism within the

³⁷ The blood quantum, or descendency clause, differs markedly among different tribes. The Western Cherokees, for example, enroll for membership anyone who is a direct descendant of a Cherokee listed on the 1907 roll, regardless of blood quantum. In contrast, the Kiowas require that all members be at least "one-quarter blood," regardless of any relative's enrollment; the Comanches require that members be at least "one-quarter blood" with at least one ancestor enrolled; and the Otoe-Missourias require that tribal members be at least "one-quarter blood, with at least one parent already enrolled in the tribe." Telephone interviews, tribal enrollment offices, Cherokee, Comanche, Kiowa, and Otoe-Missouria tribes, February 6, 1995.

³⁸ Several excellent recent studies have investigated the cultural mechanism used by tribal communities to define tribal identities and maintain tribal cohesion. See Morris W. Foster, *Being Comanche: A Social History of an Indian Community* (Tucson, Ariz., 1991); Loretta Fowler, *Arapahoe Politics, 1854–1978: Symbols in Crises of Authority* (Lincoln, Neb., 1982); and Fowler, *Shared Symbols, Contested Meanings: Gros Ventre Culture and History, 1778–1984* (Ithaca, N.Y., 1987). William E. Unrau, *Mixed-Bloods and Tribal Dissolution: Charles Curtis and the Quest for Indian Identity* (Lawrence, Kan., 1989), offers an interesting case study in acculturation and mixed-blood identity in the late nineteenth and early twentieth centuries. Also see Anderson, *Kinsmen of Another Kind*; Margaret Connell Szasz, ed., *Between Indian and White Worlds: The Cultural Broker* (Norman, Okla., 1994); and Peter Iverson's insightful *When Indians Became Cowboys: Native Peoples and Cattle Ranching in the American West* (Norman, 1994).

tribal communities. Since World War II, these attitudes have changed somewhat, but for many Native American people who passed through the boarding schools in the late nineteenth or early twentieth centuries, the institutions profoundly shaped their lives.³⁹

Because students shared classes with members from other (often distant) tribes, the experience also facilitated intertribal friendships, "boarding school marriages," and a sense of pan-tribal, Native American identity. Ironically, although the boarding schools were highly structured institutions, often disruptive of Indian family life, many of the students who attended the schools later held fond memories of them, Tsianina Lomawaima has reported. Lomawaima's study *They Called It Prairie Light: The Story of Chilocco Indian School* provides excellent insights into student experiences at one boarding school in Oklahoma, and scholars have recently examined the educational environment at several other institutions. Yet additional studies of student experiences at both boarding and public schools are needed, as are analyses of the students' careers after leaving these institutions.⁴⁰

In the twentieth century, the parameters of Native American identity have also been shaped by the urban experience. During World War II, many Native Americans left the reservations to serve in the military or to seek employment in the defense industry, and during the 1950s the Bureau of Indian Affairs' relocation program continued to encourage this exodus. Even though historians have examined the development and implementation of the relocation policies, relatively little has been written regarding the Native American experience in the cities. By 1990, almost half the Native American population of the United States lived in urban areas; and, although most of these individuals maintained ties to reservation communities, residency in cities has affected their participation in reservation affairs. Many urban Indians still vote in tribal elections, return to the reservation communities to participate in ceremonial or social occasions, and even anticipate a retirement back on the reservation. But their urban residency has fostered a new sense of community with other Native Americans who share their particular urban environment and has encouraged pan-tribal identities, which have deepened in the latter half of the twentieth century. It is not surprising that the American Indian Movement emerged in an urban setting (Minneapolis–St. Paul) or that most of its early membership came from an urban Indian

³⁹ James Axtell, "Dr. Wheelock's Little Red School House," in Axtell, *European and the Indian*, 87–109. Classic descriptions of early nineteenth-century teaching methods and curricula can be found in Isaac McCoy's reports and correspondence in the Isaac McCoy Papers, Kansas State Historical Society, Topeka, Kansas; and in Isaac McCoy, *History of Baptist Indian Missions* (1840; rpt. edn., New York, 1970). An excellent example of the racist rejection of educated, acculturated Native American students can be found in Elias Boudinot's experiences at Cornwall Academy. See Ralph Gabriel, *Elias Boudinot, Cherokee, and His America* (Norman, Okla., 1941); and Theda Perdue, ed., *Cherokee Editor: The Writings of Elias Boudinot* (Knoxville, Tenn., 1983). Also see Michael C. Coleman, *American Indian Children at School, 1850–1930* (Jackson, Miss., 1993); and Robert A. Trennert, Jr., *The Phoenix Indian School: Forced Assimilation in Arizona, 1891–1935* (Norman, 1988).

⁴⁰ K. Tsianina Lomawaima, *They Called It Prairie Light: The Story of Chilocco Indian School* (Lincoln, Neb., 1994). Also see Devon A. Mihesuah, *Cultivating the Rosebuds: The Education of Women at the Cherokee Female Seminary, 1851–1909* (Urbana, Ill., 1993); and Margaret Szasz, *Education and the American Indian: The Road to Self-Determination, 1928–1973* (Albuquerque, N.Mex., 1974).

population. Like that of the Native American urban experience, a scholarly history of the American Indian Movement is also overdue.⁴¹

In contrast to the proliferation of recent scholarship on the role of women in the broader sphere of American society, relatively few volumes have been written regarding the influence of Native American women. As Nancy Shoemaker has pointed out in her introductory essay to *Negotiators of Change*, accounts of Native American society usually were written and compiled by white men, whose own assumptions about gender precluded them from either observing or understanding the key roles that Indian women played in Native American society. Consequently, the traditional documentary evidence to support an analysis of women's activities remains limited. Shoemaker argues that, within many tribal communities, "gender was a socially constructed category, and not biologically determined." Since Native American societies were less patriarchal than their European counterparts, Indian women (and children) enjoyed more freedom. Moreover, although "Indian women (initially) had more authority and were more respected than their counterparts in Europe," their status generally declined after European or American contact. Yet, as Shoemaker and fellow essayists Theda Perdue, Clifford Trafzer, Lucy Eldersveld Murphy, Harry Kersey, and Helen Bannon show, Indian women "actively, creatively, and often successfully resisted marginality."⁴²

Because of the recent evolution of the field, and the limitations of traditional sources, much of the scholarship in Native American women's history has been concentrated in essays or journal articles.⁴³ In addition, scholars have relied heavily on interviews and oral testimony to produce biographical studies of Native American women in the twentieth century. In 1984, Gretchen Bataille and Kathleen Sands surveyed such literature in *American Indian Women: Telling Their Lives*. During the past decade, volumes such as Mark St. Pierre's *Madonna Swan: A Lakota Woman's Story*, Ruth Boyer and Narcissus Gayton's *Apache Mothers and Daughters*, and Jay Miller's *Mourning Dove* have added to this tradition, examining the lives and contributions of women within reservation societies. Native American women's history offers considerable opportunity for future scholarship. It continues to attract growing numbers of younger scholars, and, as any survey of

⁴¹ *New York Times* (March 5, 1991): A1, A10; Alison R. Bernstein, *American Indians and World War II: Toward a New Era in Indian Affairs* (Norman, Okla., 1991); Donald L. Fixico (Shawnee-Sac and Fox-Creek-Seminole), *Termination and Relocation: Federal Indian Policy, 1945-1960* (Albuquerque, N.Mex., 1986). Alan L. Sorkin, *The Urban American Indian* (Lexington, Mass., 1978), presents a brief sociological survey of the urban experience.

⁴² Nancy Shoemaker, ed., *Negotiators of Change: Historical Perspectives on Native American Women* (New York, 1994). Perdue, Trafzer, Murphy, Kersey, and Bannon have written essays in this collection.

⁴³ For example, see Kathryn E. Holland Braund, "Guardians of Tradition and Handmaidens of Change: Women's Role in Creek Economic and Social Life during the Eighteenth Century," *American Indian Quarterly*, 14 (Summer 1990): 239-58; Clara Sue Kidwell (Choctaw), "Indian Women as Cultural Mediators," *Ethnohistory*, 39 (Spring 1992): 97-107; Theda Perdue, "Cherokee Women and the Trail of Tears," *Journal of Women's History*, 1 (Spring 1989); and Helen Hornbeck Tanner, "CooCoochee: Mohawk Medicine Woman," *American Indian Culture and Research Journal*, 3 (Spring 1979): 23-42. Also see Patricia Albers and Beatrice Medicine, eds., *The Hidden Half: Studies of Plains Indian Women* (Lanham, Md., 1983). In contrast, an excellent monographic study of Native American women can be found in Sylvia Van Kirk, *"Many Tender Ties": Women in Fur-Trade Society in Western Canada, 1670-1870* (Norman, Okla., 1980). Also see Carol Devens, *Countering Colonization: Native American Women and Great Lakes Missions, 1630-1900* (Berkeley, Calif., 1992).

contemporary Native American politics illustrates, Native American women still exert a powerful influence within their communities.⁴⁴

THE ISSUE OF WHO should exercise the appropriate "Indian voice" currently looms on the horizon of modern scholarship. Partially as a result of the repatriation controversy, and also motivated by Native American concerns over the usurpation and commercialization of sacred objects and ceremonies, both Indian academics and political leaders have recently questioned the content, methodology, and even the purpose of Native American history. In an essay, Donald A. Grinde, Jr., a Yamasee scholar, has argued that "the image of the American Indian in history, literature, and art has been largely an 'invented' tradition external to the American Indian experience . . . Native American people often find their history imprisoned by the rhetoric and scholarly inventions of empire."⁴⁵ Other Native American historians have accused academics of extracting privileged information from Native American informants, then using such material for their own purpose, while callously disregarding the sensitivities of the tribal communities. Moreover, many are offended by historians and anthropologists who assume self-appointed roles as "caretakers of tribal histories and cultural knowledge" and who "claim that Indians are too witless to chronicle their own histories."⁴⁶

At issue in this debate is a conflict over "voices" and audiences. Indians have repeatedly claimed that much of "academic" Indian history does not reflect a Native American perspective; it reflects only what non-Indian academics think is important in the lives of Indian people. For example, when historians have written about Native American history on the plains during 1833, they emphasize events such as intertribal warfare, the fur trade, a cholera epidemic, or monumental floods along the Arkansas River. In contrast, the winter counts, or pictographic calendars recorded by the Great Plains tribes, for 1833 focus primarily on a spectacular shower of meteors that fell to earth during the evening of November 12, and the plains people remember this time as "the winter that the stars fell." Many people in tribal communities want a Native American history that focuses on the parts of their life or the lives of their grandparents of interest to them, and historians on the faculties at tribal community colleges or larger institutions with Native American Studies programs responsive to local communities have produced works that do so. Many of these histories are admirably designed for the

⁴⁴ Gretchen M. Bataille and Kathleen Mullen Sands, *American Indian Women: Telling Their Lives* (Lincoln, Neb., 1984); Mark St. Pierre, *Madonna Swan: A Lakota Woman's Story* (Norman, Okla., 1991); Ruth McDonald Boyer and Narcissus Duffy Gayton, *Apache Mothers and Daughters: Four Generations of a Family* (Norman, 1992); and Jay Miller, ed., *Mourning Dove: A Salishan Autobiography* (Lincoln, 1990). Research conducted by anthropologist Loretta Fowler has confirmed the continued influence of women in tribal politics in Oklahoma. See Fowler, "Oklahoma Arapaho Chieftainship: Rethinking Cultural Perspectives in Ethnohistory," a paper read at the Annual Meeting of the American Society for Ethnohistory, 1988.

⁴⁵ Donald A. Grinde, Jr. (Yamasee), "Teaching American Indian History: A Native American Voice," *Perspectives*, 32 (September 1994): 11-12.

⁴⁶ Devon A. Mihesuah (Choctaw), "Suggested Guidelines for Institutions with Scholars Who Conduct Research on American Indians," *American Indian Culture and Research Journal*, 17 (1993): 132.

needs of the audiences for which they have been produced, but some have been rejected by other academic historians. In this instance, tribal historians and scholars addressing their studies to particular Native American audiences may share problems similar to those of public historians.⁴⁷

Closely related to the issue of audience is the question of an "Indian voice." Ideally, an Indian voice should address those historical questions considered important by Indian communities but should also present a Native American perspective, even on historical issues that may be of secondary interest to most tribal people but that have been, or are, championed by non-Indians. Of course, any discussion of an "Indian voice" assumes a perspective shared by most Indians, and in many situations Native American opinions have been so diverse that to attain a representative, unified voice is difficult, if not impossible. Yet almost all Native American historians, academic and otherwise, would agree that historical accounts and analyses of Indian people would be enhanced by the inclusion of the perspectives of the people participating in them. Do historians who are members of the tribal communities possess particular insight into these historical issues? Are their insights into recent events much more valid than those in the distant past? Can historians (non-Indian) who are not members of the tribal communities speak with an "Indian voice"? If they are thoroughly familiar with tribal communities and have conducted careful research, can they infuse their work with a tribal or Native American perspective?

These are difficult questions, and both Native Americans and academic historians differ with each other, and among themselves, over the proper response. Most academic historians who are not members of tribal communities but who have devoted their professional careers to the pursuit of Native American history would hesitate to assert that they write (or present their research) with an "Indian voice." But most would also argue that they have attempted (with mixed success) to include a Native American perspective in their work. Obviously, as Donald Grinde points out, most academic historians do approach their subjects within the framework of traditional European or American methodology, and the parameters of that methodology ("the rhetoric and scholarly inventions of empire") continue to shape their inquiry. Yet the fruits of that inquiry (books, essays, and documents written or edited by academics), even in the hegemonic strictures of Foucauldian interpretation, now provide a basis for either revision or further investigation by a growing number of young, talented Native American historians who have emerged from the tribal communities. Moreover, insights provided by these scholars have both enlarged the more traditional academic perspective and have generated considerable debate. In contrast, the danger of the "every group its own historian" approach is the potential atomization of scholarship and the failure of different camps even to communicate. Perhaps the best of all possible worlds is, to paraphrase Richard White, a method of inquiry in

⁴⁷ Louise Barry, *The Beginning of the West: Annals of the Kansas Gateway to the American West, 1540–1854* (Topeka, Kan., 1972), 251–52; Garrick Mallory, *Pictographs of the American Indian, 4th Annual Report of the Bureau of American Ethnology* (Washington, D.C., 1886), 116. For the dilemma of public historians and an analysis of the problems encountered in the formulation and writing of African-American and women's history, see Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, 1988).

which Native American and non-Indian perspectives and methodologies “melt at the edge and merge” and in which it becomes unclear “whether a particular practice or way of doing things” is Native American or non-Indian. Certainly, Grinde’s suggestion that non-Indian historians immerse themselves in Native American languages is a logical step toward achieving this process.⁴⁸

Since this essay began with a general lament over the historical profession’s former dismissal of Native American history, and an enumeration and complaint about the paucity of articles or essays focusing on Indian history published in the *American Historical Review* during the first sixty years of its existence, perhaps it should end on a happier note. Within the past two decades, Native American historiography has seen explosive growth, and publishers are actively seeking worthwhile manuscripts for their lists. The University of Oklahoma Press continues to add to its series, but it has been joined by university presses from Nebraska, Arizona, New Mexico, North Carolina, Indiana, and Illinois, as well as several other academic and commercial publishers who now recruit and publish monographs. Older established scholarly journals have published a growing number of essays focusing on native subjects, while several new journals dedicated to Native American history and culture have emerged. Meanwhile, expanding enrollments in Indian history courses or Native American Studies programs have engendered several textbooks that have found wide acceptance.⁴⁹

The table of contents for the *American Historical Review* during the past two decades reflects such changes. In addition to this essay, since 1975 the *AHR* has published two review essays, one presidential address from Charles Gibson, and two scholarly articles that focus on Native American history. In 1975, Wilbur Jacobs’s essay, “Native American History: How It Illuminates Our Past,” examined many of the books that were emerging as part of the new Indian history and argued that they “threw a fresh beam of light on general American history.” Twelve years later, James Axtell surveyed American history textbooks in “Europeans, Indians, and the Age of Discovery in American History Textbooks” and demonstrated that many of these volumes continued to ignore or misrepresent Native Americans during this period. Charles Gibson’s Presidential Address, “Conquest, Capitulation, and Indian Treaties,” examined the nature of diplomatic agreements between Indian nations and Spain, while Alden Vaughan’s essay, “From White Man to Redskin: Changing Anglo-American Perceptions of the American Indian,” pointed out that the English originally believed that Native Americans would be integrated into a European-dominated society and did not

⁴⁸ Grinde, “Teaching American Indian History,” 11–16. Also see James Axtell, “The Scholar’s Obligations to Native People,” in Axtell, *After Columbus: Essays in the Ethnohistory of Colonial North America* (New York, 1988), 244–53; Frederick Hoxie, “The Problem of Indian History,” *Social Science Journal*, 25 (1988): 389–99; Novick, *That Noble Dream*, chap. 14; White, *Middle Ground*, 50.

⁴⁹ The *American Indian Historian* began publication in 1964; the *American Indian Quarterly* in 1974; and the *American Indian Culture and Research Journal* in 1974. Textbooks currently include Arrell M. Gibson, *The American Indian: Prehistory to the Present* (Lexington, Mass., 1980); William T. Hagan, *American Indians*, 3d edn. (Chicago, 1993); Albert L. Hurtado and Peter Iverson, eds., *Major Problems in American Indian History* (Lexington, 1994); Roger L. Nichols, *The American Indian: Past and Present*, 4th edn. (New York, 1992); Frederick Hoxie, ed., *Indians in American History* (Chicago, 1984); R. David Edmunds, ed., *American Indian Leaders: Studies in Diversity* (Lincoln, Neb., 1980); Donald L. Parman, *Indians and the American West in the Twentieth Century* (Bloomington, Ind., 1994).

focus on Native Americans' skin color. During the eighteenth century, however, after the English began to have doubts about assimilating the Indians, they increasingly described Native Americans as "inherently inferior 'redmen'" who were "prevented by 'nature' rather than education or environment from full participation in American society." In 1991, Melissa Meyer's article, "'We Can Not Get a Living as We Used To': Dispossession of the White Earth Anishinaabeg, 1889–1920," demonstrated that Chippewa tribespeople used the White Earth Reservation as a "region of refuge," where they practiced diverse economic activities that enabled them to slowly adjust to a market economy. When federal officials succumbed to state and local lumber interests and permitted the premature sale of individual allotments, much of the reservation land base was lost and the Chippewas were somewhat assimilated but were "marginalized" in the American economy. Yet, more important than the publication of these articles, the decision by the editors of the *AHR* to include an essay on the growth and development of Native American history in their centennial issue reflects the acceptance of this field as a crucial part of American history. It is difficult to imagine the inclusion of such an essay prior to 1970.⁵⁰

In 1895, the mounted warrior in Fraser's "End of the Trail" was portrayed as symbolic of all Native Americans: tragic figures, associated with the past, and descending into oblivion. One century later, such an assessment has proven wrong. Indian people continue as a viable part of American society. A renewed interest in their past tells us much about the American experience.

⁵⁰ Wilbur R. Jacobs, "Native American History: How It Illuminates Our Past," *AHR*, 80 (June 1975): 595–609; Charles Gibson, "Conquest, Capitulation, and Indian Treaties," *AHR*, 83 (February 1978): 1–15; Alden T. Vaughan, "From White Man to Redskin: Changing Anglo-American Perceptions of the American Indian," *AHR*, 87 (October 1982): 917–53; Axtell, "Europeans, Indians, and the Age of Discovery in American History Textbooks"; and Melissa L. Meyer, "'We Can Not Get a Living as We Used To': Dispossession and the White Earth Anishinaabeg, 1889–1920," *AHR*, 96 (April 1991): 368–94.

Historical Truth: An Interview with Ken Burns

THOMAS CRIPPS

Over a year and a half ago, the editor of the *AHR* extended an invitation to Ken Burns to write an essay for the centennial issue of the journal. Burns was asked to discuss the changing nature of historical truth during the course of the century following the first issue of the *AHR*. When the press of his film work prevented Burns from contributing an original piece of writing, he agreed to be interviewed, and David Ransel sought my services as the interviewer.

After a brief negotiation by telephone, we agreed to meet in the afternoon of November 17, 1995, at the Wurst Haus in Harvard Square, from which it was an easy walk to the Afro-American Studies Department of Harvard University, in whose departmental library the interview took place. The cassette-recorded conversation took most of an afternoon, during which Burns allowed far more time than originally promised. The recording was transcribed by the staff of the *AHR* in Bloomington, Indiana, and a copy sent to Burns's staff.

Ken Burns confesses to an almost devout fondness for American history, a love that, coupled with his thorough research on film projects, justifies his proud claim to the status of "amateur historian." A mature man of youthful appearance, Burns seems to relish the role of provocateur of the academic establishment. Yet he is quick to point out the deep trust and reliance he places in the professional historians who sit on his boards of advisers. It is this combination of faith and skepticism from which his sense of historical truth springs.

While he admires and trusts the intellectual achievement of academic historians, he criticizes them for having abandoned their role as tribal storytellers who craft tales about the past in which the nation can find its identity. The gulf between these two attitudes toward the profession of historian defines the difference between two kinds of historical truth.

Burns's own sense of mission as storyteller is associated with his ideas about the accomplishments of documentary film. Parallel to a century-long professionalization of history, the documentary film has grown from generations of dull "travel" film and "educational" films, leavened only by a few heroic accomplishments such as Robert Flaherty's *Nanook of the North*, Pare Lorentz's *Plow That Broke the Plains*, and William Wellman's Oscar-winning *The Memphis Belle*.

Burns's sense of the past thus derives from two sources: his own sense of self as "amateur" historian as well as a sense that documentary film is at the height of its powers. The resulting sensibility and achievement has given him an audience that few historians who work in the printed word will ever reach. As producer, director,

cameraman, and writer—often simultaneously—he has shared in the creation of films on the Brooklyn Bridge, Huey Long, the Statue of Liberty, the Shakers, and more, including, most famously, *The Civil War* and *Baseball*. Together, they have won or been nominated for dozens of awards, including Oscars, Emmys, and Peabodys.

Burns, born in Brooklyn, studied film and design at Hampshire College. He is co-founder of Florentine Films and lives in Walpole, New Hampshire, with his family.

Thomas Cripps: Let me say one brief word about what we are doing. This is the hundredth anniversary issue of the *American Historical Review*, and the idea is to inquire into the changing nature of historical truth [over the past century up to the present]. That is a direct quote—“historical truth” [from the charge given me by the editor], right? And a medium outside of print has intruded in those hundred years.

Ken Burns: Yes, yes.

TC: And you were the person—

KB: —who has most intruded.

TC: Clearly so, although I am going to remind you . . . of some people who have tried in the past with considerably less success than you . . . That’s the sense of it. And you were asked through [your assistant], I am sure, to write this, and you said no, that you hadn’t the time, and other factors prevented you from doing it. So they [the *AHR*] held still for the interview as the format.

KB: Right, they did. I’m very grateful for that, and only because I have not had a moment to myself since the last . . . [Though] I’m a good writer. I love to sit down and write and craft a speech.

TC: But you have to seal yourself off—

KB: Right, and I have not even been able to do that.

TC: I would like to have a sense of what you feel about the differences between what you do and what someone does in a book. Let’s say Bruce Catton [author of a trilogy on the Army of the Potomac], to speak of a popular historian.

KB: I’m not sure there’s too much difference in the final product or . . . the original intention. I think we’re seeking the same kind of communication with a broad audience to, some would say, rescue history from those who teach it and the scholars who only wish to talk to themselves about it, and to return history to kind of a broad dialogue. How you get there, though, is so terrifically different, I think, than the written word. I mean, at the end of the Civil War, Walt Whitman said,

“Future years will never know the seething hell, the black infernal background, the countless minor scenes and interiors of the secession war, and it is best they should not.” The real war, he thought, would never get in the books. And it’s a really wonderful phrase and a kind of a challenge to us all to consider how we might translate that black infernal background. And I felt that, in a way, it was impossible in a book to get all of the Civil War in, that words were not quite enough in this instance. That you needed them, they were central, as words are central to my film [*The Civil War*, 1989]. They are first written. It begins with the word. But, after that, it was important to build other dimensions of understanding, that it was possible to have a historical relationship—an honorable historical relationship—with, say, music, with the exploration of still photographs, with sound effects, with a kind of [*pause*] time-released [*effect*]. That’s what film allows. It orders you [*the viewer*] into a particular moment of reception. [*On film*], I only allow you a certain amount of time with this photograph, whereas in a book you could linger over a paragraph or an image that illustrates that paragraph . . . for as long as you wanted.

TC: Now when you speak of reception, suppose it were an either/or question. Do you want to raise an emotional response in a person or an intellectual one? Or, another way of phrasing that is, which does that more, television or print?

KB: Well, they’re both remarkable mediums for emotional response. I still think the greatest man-made machine in the world is the book, and in no way would I hope to supplant it. I think they are vehicles for terrific emotional understanding and communication. Film, really, is lacking for rigorous intellectual communication. It just doesn’t have the tools because of its temporal nature. That is to say, it has a duration. It’s actually beholden to Aristotelian poetics. I would like to leave the marketplace out of this, because I have the good fortune to exist outside the marketplace with public television. I don’t think the demands for it being fast-paced or commercially viable really apply in my case. But I do know [*that*] the medium itself, the actual constructions of films require in some ways an almost anti-intellectual posture. You cannot get that much . . . into it; you really have a difficult [*task*] because of this time thing—just [*as*] you could say a ballet or a symphony does not have the ability to carry complex intellectual thought. Very emotional, very visceral, very visual—[*it’s*] the same with film.

TC: Let me give you a difference between print and television that I would perceive, and maybe we’ll get [*yet*] another spin on that. In a book, you have the opportunity to qualify, challenge, and put another point, even as you are making a point.

KB: Yes, that’s correct.

TC: Lincoln was the “Great Emancipator” but did not arrive at that [*stage*] until very late in the war. And, therefore, there is an ambivalence that you either can’t or won’t be able to get into a film.

KB: I disagree wholeheartedly. First of all, I think that ambivalence and the tolerance of that kind of opposing view is in fact what film is particularly suited for. Let me speak to *Civil War*, for example. Not only does our Civil War film include all of the things you just said about Lincoln and do so with stunning clarity: “He is a first-rate second-rate man” is what Wendell Phillips said, and we quote, in the film. But in fact I have no example of a Civil War *book* that actually tolerates as many diverse viewpoints as our film does, that is to say, gives a Southerner life without accepting in any way a Southern point of view. It gives [attention to] ordinary Northern soldiers, which is a particular school or way of telling history, as well as [to] the kind of top-down version [of history]. It is one that includes a discussion of emancipation and abolition but does not neglect the battles—which are out of fashion in many, many books. It actually, I believe, shows Lincoln in his complexity. This is a film in which Nathan Bedford Forrest is actually appreciated for his military skill without ever once being let off the hook for his previous slave trading and his future participation in the Ku Klux Klan. This is a film that has only two villains: slavery and [General] George Brinton McClellan. And yet, it raises the [issue] that, as McClellan says, “the sight”—and I won’t get this quote exactly right—“the sight of broken and maimed bodies on the battlefield holds no glory for me”—which is a wonderful phrase . . .

I believe that the film has the possibility to actually include *more* points of view. The problem is that if it is responded to with a narrow, polemical—[whether] academic polemical, political polemical, politically correct polemical—point of view, then *every* point is seen as a particular advocacy, rather than merely [its] inclusion. So if you watch the popular response to *The Civil War* or the *Baseball* series, you have essentially 98 percent saying, “This is fabulous,” and you’ve got 1 percent on either end saying . . . , “This is a pro-black film and you’ve left the Southerners out,” and others saying, “This is pro-South and you’ve left out the complicated political story and you’ve left out the social transformation of the country and you haven’t done enough with emancipation.” And yet the huge, broad, popular response from artists and ordinary people has been, “How did you get it all in?” It’s only in the academic community that I found a particularly—and for me a sad—painful sort of rejection. So then one has to say, “Is this jealousy, is it, you know, that I’m not hewing entirely to one point of view?” That film was able to encompass *many* points of view.

TC: I don’t think of it as jealousy, and I haven’t heard that as uniformly as you’ve made it seem. I find a lot of [historians] embracing the [*Civil War*] series.

KB: In the academic community, in the university. But there’ve been some quite vocal criticisms that we didn’t do things, . . . [and I can only say], “Did they actually see the film? Did they look at the first chapter of the film?”—which is a dramatic, I think, one of the most dramatic, indictments of slavery that has ever been on film.

TC: I think what they are after, perhaps, is the notion that you have to grasp at some important document that speaks visually. And in doing that, you only have

that few seconds, plus whatever [sound]track you put on, whereas they [writers] have hundreds of documents.

KB: I think it is possible to realize the limitations of the form, just as I hope they realize the limitations of their own form. Not just within the printed word—literature—but within an academic or a scholarly relationship to a subject—there are inherent limitations there . . . I mean, this [film] is quite different from a book, and from a textbook.

TC: Try to get at what that distinction is—that is, *their* limitations. The reason I'm raising this is that those limitations were defining and dominant a hundred years ago, because there was no television.

KB: Those limitations, I would suggest, weren't entirely in there, because I think one of the limitations that has beset the historical community has been the adoption of this kind of Germanic, for lack of a better word, academic model, which has imposed on its scholars the necessity to speak only to themselves in language which is rarely readable for the general public, and so render our history away from the people who most benefit from history. It is now anti-narrative for many of these people, [although] the word "history" is mostly made up of the word "story." It has no mandate to speak to a large group of people. And so the effect of that, I believe, has been to [leave] not just students but, in the trickle-down theory of historical interest, *everyone* disinterested in where they have come from. And I think that is the single greatest crime that historians perform daily when they deny tenure to those who write for popular magazines. I have a friend at [an institution] who was fearful of writing for a popular historical magazine lest he be denied tenure. That kind of, kind of tyranny, I think, defeats the whole purpose of history. So that is one factor in which the academy and its sanctioned mode of expression, the paper or the book, has a limitation which I think is absolutely devastating to the historical memory of our people.

TC: Without asking you for a bibliography, are there historians [who] you find either break that academic mold or you've found so useful . . . ?

KB: Oh, all the time. Absolutely. I'm an amateur historian. I'm a popular amateur historian. That is what I do secondarily. I'm first and foremost a filmmaker. I make films. I'm an artist. That's what I consider myself. I happen to use history, American history, the way someone might choose to use oils as opposed to charcoal or watercolor. See, first and foremost, I'm actually using things that went on in the past in a very personal and emotional interpretive process, though I begin all of my work [by] going to scholars. Every single film has been attended [to] by the most remarkable set of scholars—men who are mentors to me, quite apart from people in the film community. I have mentors in that community but also C. Vann Woodward, Robert Penn Warren, Lewis Mumford, William Leuchtenburg, Alan Brinkley, people who have served on not just one but many of the films and are there from the original conception and who shake their heads with

a kind of disappointed sense when their own colleagues miss the point and [then] use films, which are very convenient targets, for sometimes advancing their own . . . narrow points of view or schools of thought or relative truths.

TC: Speaking of schools of thought, in having a popular audience—this is not to say you *aim* at a popular audience; that's not the point—

KB: No, I make the films for myself.

TC: Yes, I understand that. But do you find yourself in a school?

KB: No. I actually was so excited that film had this ability to straddle, encompass, contain many points of view—that . . . you could go into the orchard of historical inquiry and actually fill your apron up with ripe fruit from many different trees and come back and contain them [include them in your work]. [Thus] for Alan Brinkley, who had spent much of his professional life considering Huey Long, the film that I made [*Huey Long*, 1987], whose script could be reduced to twenty pages, was, in fact, a revelation, and it gave him a dimension on Long that he had yet to apprehend in his own work. That [film] could, in *The Civil War*, take a top-down version, that is, exalt the Lincolns and the Lees, the Grants and the Jacksons and the Shermans, [who] had been lost to a broad popular audience through the pernicious, insidious history of *Gone with the Wind* [David O. Selznick, 1939] and *Birth of a Nation* [D. W. Griffith, 1915] and from a country that's just not interested in those things anymore. But, at the same time, [film could] suggest that at the central core of it was a yearning for a new birth of freedom by a group of people who had been enslaved for 350 years on the [American] continent. That it could communicate an experience of war at a level felt by ordinary soldiers on both sides and by newly freed black men. That women and their point of view could be contained [included], and that there could be dimension, indeed, complexity and ambiguity to some of the greatest heroes who may have existed heretofore in a broad popular imagination as singularly one thing, singularly good or singularly bad. That John Brown could be a complex figure [who] could be considered by some a guiding light and a revolutionary, by others a base murderer or a lunatic. Or that the film could contain the multitudes that Whitman thought *he* could. And that's exciting to me.

TC: What [part] of that vision you have of filmmaking would you consider the most personal? Something that *you* do. Not what other historians don't do but rather what other filmmakers don't do. In other words, what's your contribution to the medium as a person, or a personal practitioner?

KB: I think it's an absolutely undying love of my country combined with a, what I've called an "emotional archaeology" that I'm engaged in. That is to say, I feel that the facts, the dates, the events, even the ideas, the intellectual matter of history, are best not only conveyed but absorbed and kept by me and by an audience when they come with certain emotional associations that I wish to

excavate along with the facts of the past. That they become the glue that keeps things together. And I think that is why people respond. The first [notion] can be reduced simply [by saying] that there is more *unum* than *pluribus* in my work, that I look for the “mystic chords of memory,” and that the stuff of the work is unafraid of an emotional content, which I think many scholars are afraid of. That if you take a personal diary quote, it can only be denuded for its statistical demographics rather than for an emotional content. What is that *saying*?

TC: There is another angle to that that I wanted to ask you later, but it is also appropriate now. Historians, the sort you are speaking of—academics—see themselves as moving beyond what they think of as an earlier era of “consensus” history. That is their word. Yet when you say your work is more *unum* than *pluribus*, in effect you are saying that your medium and your style rest on concepts regarded as old-fashioned by historians.

KB: I agree. I think that’s the primary misinterpretation: that they believe I am seeking a consensus history, an old version of the past which had been rejected for being too safe, too pat, wrong in many instances, filled with myth. But they’ve missed the point. The consensus I seek is an emotional consensus that comes from . . . lifting up the rug of history and sweeping out some of that dirt. So I’m often wounded, hurt, overreact to criticism, because I see them not seeing what the film is about, . . . To hear [for example] someone say, “Well, it’s the same old military stuff.” Well, first of all, in broad popular terms, there *is* no military stuff. When I went to school, I was taught causes and . . . then the next day I was taught effects of the Civil War. And I asked, “Well, what happened?” “No,” they said, “This is ‘old’ history, and if you want to get that, go down the hall to old Professor so-and-so”—as if the death of 620,000 Americans did not matter! There are battles in that war in which their outcome, if they had been different, would spell incredibly difficult things. But if you want to analyze—statistically—take apart *The Civil War* series, it’s 40 percent battle! It is 60 percent the stuff that I have been accused of not having.

And so I have wondered: among those critics who have actually watched it, . . . are they a) aware of the phenomenal success it had among a population [buying] books (not *our* book but everyone else’s books about the Civil War); that it has sponsored [inspired] trips to battle sites and museums in which the Civil War is considered in greater detail; and that it is, in fact, able to contain many different points of view; that when I include a Southerner saying that “our” whole theory of slavery is wrong, how is this sympathetic to the South? You know, when you see Wendell Phillips and [historian] Barbara Fields *in tandem* excoriating Lincoln for his tardiness, how [has] this neglected the complexity of the Emancipation Question? When we say at the end that it would be decades before blacks would gain any of the rights for which so many people had supposedly given their lives, how is this a failure? Eric Foner, for one, who was a consultant to the project, said that he was disappointed that our “ninth inning” didn’t do Reconstruction. Well, I didn’t *do* Reconstruction. I was doing the Civil War. That’s what I chose. I’m happy, I’d love, to do a film on Reconstruction, but you can’t criticize it [*The Civil*

War] for not having done what it is, because then if you do Reconstruction, wouldn't that [the next] failure be Jim Crowism and the period after that, and wouldn't therefore . . . failing to include Jim Crow be a failing of a now huge film? I mean, it's an impossible burden to carry.

TC: To what extent would you reenact something?

KB: Well, I say that I don't do reenactments. However, I had a horse gallop across a puddle. I have first-person voices read diaries and journals, letters, military orders, newspaper accounts—[all] off-camera. I have people let us into buildings before they are open to the public, and I filmed a shrine where Stonewall Jackson died. These are, in essence, reconstructions. I take old photographs and do that. But any historian is doing a re-creation, in a way. They are calling together some facts; they are ordering them in a specific way. I've set some limits: I don't wish to have actors and actresses portraying people and reading lines until I do a feature film.

TC: I was going to spring *Nanook of the North* [1922] on you. He [Nanook] was encouraged [by director Robert Flaherty] to repeat acts that he routinely did but in a [summer] season, when it was less threatening.

KB: I have been accused by historians of actually placing words into the mouths of my so-called "talking heads," so that they will say what I want them to say, and I have never once—

TC: Oh, you mean contemporary ones, like Barbara Fields or some other historian?—

KB: Yeah. And I have never once asked a question over again in that way. When we ask a question over again, it's because we've had a technical problem—we've run out of film or the sound went off, and we say, "Could you say that again?" But we've never said, "We want you to say something like this." I go to an interview with a wide range of questions and then cut it down . . . That's a form of re-creation as well.

TC: Oh, yes. It's like choosing a page or a line in the document.

KB: Yeah. I think what happens is there [are] too many separate truths that the academy has, and that it is important *not* to find a consensus which is a bland and homogenized version of our past that would be satisfying to all. That is not what it [our past] is. But we would seek to find a way in which we can include the diverse tributaries of our experience into something that might nourish the whole. And that's a difference. And that one might be suspicious of a process that is quite different is merely a kind of academic and artistic xenophobia. It is a fear of others doing things differently, and that's a parochialism that the academy can well do without. Plus, I think it is, once again, the fear of good language—the fear that if

you *are* speaking to a broad number of people, you can't be doing a good job. And that is certainly poppycock!

TC: But I think it [the encouragement of difference] sometimes guides us.

KB: Oh, it absolutely does! And I'm going to rail against it. I believe that history ought to be sung, that Homer, the Homeric mode is an important one, that you need to sing the epic verses, and the way we do that is around this "electronic campfire" [television]. And it is an inclusion of myth as well as fact, because myth tells you much more than fact about a people. But you have to *distinguish* between myth and fact. As our *Baseball* film begins with re-creations of ball players in silhouette playing a game in which we propose the Abner Doubleday myth [of the invention of the game in Cooperstown, New York] and then take it away and never do a re-creation [of an event] for the rest of the eighteen and a half hours. It was my way of saying that history does exist in this nostalgic vacuum and that what we need to do is open it up to a much more complicated story of—not this pastoral game invented by Abner Doubleday, who never saw a professional baseball game in his life—but a gritty New York story, born near saloons. You know? And really, in essence, a story about race—which is what my eighteen and a half hours [is about] . . . *Baseball* is a sequel to *The Civil War*—they are all a meditation on race in America. And I am now doing a film on Thomas Jefferson—getting away from the Sally Hemings story, for which there is no proof—and digging into the fact that this is the man who distilled the essence of the Enlightenment, a hundred years of Enlightenment thinking, into one remarkable sentence that begins, "We hold these truths to be self-evident; that all men are created equal." And yet he owned a hundred human beings who he never saw fit in his lifetime to manumit. That's Jefferson! . . . [Not] the . . . scandal of Sally Hemings! *It's unprovable*, and it's a sexual distraction from the *real* politics of race [that history] ought to be about. Those like [Professors Leon] Litwack and [Daniel] Walkowitz, who ought to be the film's most ardent supporters because of [its] way of getting a message that is so closely allied with them out to a broad public, and they need only come in and pore through the letters from people about the Negro Leagues or about the Civil War to find the kind of subtle transformation of old beliefs falling away in the face of evidence of African-American activism during the Civil War to know that their criticisms miss the point.

TC: I don't know their criticisms specifically.

KB: They're quite pointed and, unfortunately, miss the point.

TC: But they've written this somewhere?

KB: Yes.

TC: I missed that. This is continuing with becoming an academy versus—

KB: Which I don't mean to be, because, you know, I have more friends in the academy than I do in filmmaking. I spend most of my time with scholars—and they help. I mean, Bill Leuchtenburg has been so incredibly central to the success of the last two films, in his careful and patient guidance. Gerald Early, who directs the African-American Studies Department at Washington University [in St. Louis], I think helped us more than anybody else in understanding how this African-American narrative permeates our film in every episode of *Baseball*. We cannot do this. We begin with a body of historical advisers and work with them—and all of whom I think write well, all of whom I think are frustrated with the Babbitry in the aspect of the academy that we've been talking about. But let's say it is only an aspect. I find C. Vann Woodward not just one of the finest scholars in this country but one of this country's most beautiful writers, and an artist.

TC: . . . What about sponsorship? Any academic can say that his work is untrammelled by sponsorship—partly, perhaps, because of the lower stakes: quite often, a university press may expect a very low sale for an extremely important book but one meant to be read only by one's [specialist] colleagues.

KB: It has no effect on what I do. I'm in public television for two reasons. Nobody can tell me what to do. That is to say, I own my film and I control its distribution, and no sponsor can tell me what to put into it. That's one. The other is, there is no commercial interruption.

TC: But there are *sponsors*, though, in the sense that—

KB: Yes, but they are absolutely—by the conditions of public television—insulated from any involvement in the content of the film. General Motors does not tell me how to make films, and I do not tell them how to make cars.

TC: And there would be no preview for them—or a rough cut?

KB: I made a film on the Statue of Liberty that was underwritten by an insurance company, [a film] in which James Baldwin said, "The Statue of Liberty means *nothing* to me; I have utter contempt [for it]." I mean, I'm sure they swallowed hard, but they cannot influence it in any way. There is more control from the National Endowment for the Humanities, [which] requires [a] rigorous proposal process and scholarly intervention.

TC: Speaking of the National Endowment, in academic writing, one's editor is a scrutinizer who will challenge (and of course there are readers who challenge) [one's] work. Often, it results in rewriting in considerable amounts. To what extent might the NEH oblige you to change something, something that's not—

KB: They may say in a proposal process—this is why I suggested that they may have more control than anyone else does, [though] I have ultimate control—but they can say, "We think that this proposal is leaning too far and there is not enough

balance in it or you haven't engaged a [particular] scholar." They may make a suggestion like that. They have never had to do that in my case because I've come to them prepared with the scholars. I know they have probably done it in others. Once the process begins, I am influenced by these very editors—the board of scholars that we have, my editors and colleagues making the film . . . We sit and talk about that [balance issue], and I make decisions based on "You've gone too far here," or "You really can't say that," or "The evidence proves that it's not that," or "I really think that we're lacking the kind of overview of what's going on in the country at large. I mean, as we talk about segregation in baseball, don't we need to also intimate the Harlem Renaissance?" Yes, we should—and we might rewrite. No, we shouldn't—it's staying the way it is. And so, I'm the final arbiter of that. But, at the same time, I have in many ways *more* editors than a single scholar would [have]. And, at the same time, I think I have more complete freedom to do that.

TC: So, you wouldn't imagine yourself as having a kind of self-censorship because you [anticipate] what someone might [say], such as the NEH or—

KB: No, never, that never even crossed my mind until you asked this question. In fact, I've chosen to work in public television because it is the freest place. There are, through the various funding mechanisms and agencies of public television, [sources] where you can get money to give you the kind of insulation, if you are fortunate enough to be funded, to do this. The system itself is designed to protect, particularly, I feel, in historical inquiry, the historian from any kind of outside influence by corporate sponsors, foundation sponsors, and government sponsors. Look, I've made two of the longest films in public television's history; they are a non-standard length, they are [*pause*] decidedly personal, artistic works, and no one has once said, "I think you should change something."

TC: I had a question that was going to lead to that, toward "political correctness," but I'm going to let that drop because I think you've said—

KB: Well, I have many points of view about that!

TC: And I think you've made quite a few of them—

KB: Like all good things, multiculturalism and political correctness have really powerfully important things [to say], and like all good things they also have very bad and pernicious side effects.

TC: Yes, yes, I think we are beginning to see that to an extent . . . Let me try out one more thing that is sort of general.

KB: Is this helpful? Am I being helpful to you today?

TC: Oh, yes, yes, yes, please feel that way. This is a reworking, in a sense, but I just was speaking with a man who has produced some things for PBS. We had both

been addressing a group of . . . archivists, and he was saying that he buys—what he meant was that he chooses, but he used that verb “buy,” so that we would understand that money goes into [TV], and it can’t be just tossed away. Anyway, he said he buys footage for scene-setting, emotional impact, and only thirdly for what he thought of as the facts of the matter. That is, in a way, putting a spin on something you have already said about how important the emotional content linking—

KB: For anyone trying to make something historical . . . whether it’s a scholar or a filmmaker, there is an exquisite and sometimes excruciating tension between art and science. That is to say, the science would enumerate the myriad details equally, without discrimination. [For instance], you could get to understand Cambridge, Massachusetts, by reading the telephone book. [But] the other, *artistic* side has produced some of the greatest cinematic crimes, for example, you know, written screeds, *The Protocols of the Elders of Zion*, or *Birth of a Nation*. That it is in this middle distance that we even in scholarly work adjudicate all of these questions. I think what that filmmaker is saying is that there are tough choices that filmmakers make [between fact and emotion]. I would not go so far as to say that the *fact* is a secondary, or even tertiary, part of it. What it is is an exquisite tension in which, in many cases, not all, you may be making a choice that you would consider—as a scholar—poetic license of a degree that you couldn’t take [accept]. That would mean that fact had been relegated to a secondary position. However, there are an equal number of times when the fact of something overrules a much better artistic presentation. The sum total, I think, may be to the finely tuned scholar more egregious on the whole. I mean, let’s stop for a second and realize that more than a million images, photographs, were taken during the Civil War with this new, developing medium of photography. Not *one* of them is a photograph of battle.

TC: Or even movement.

KB: Even movement. But I mean of battle itself. So that means that every shot that I used [of] battle is a lie [from] a fundamentalist point of view. At the same time, I would suggest that the possibility to understand the war was, by our country, improved immeasurably in a broad way by an experience of that film. I just *know* that—and I’m not being immodest. I’m just saying I know that this had a profound effect on our country. Millions of people have seen it—sixty, seventy million people have seen this. Battlefield [site] attendance went up; the purchase of books of all stripes, from scholarly works to broad popular works, went through the roof. Shelby Foote sold more in six months of his 3,000 pages of history than he had in [their] previous fifteen years [in print]. So what I am trying to say is that there are huge choices that you make, but I would say we come to each moment, each decision, not thinking of priorities—or at least I don’t . . .

TC: I want to go back . . . We were talking about reenactments, and one thing I wanted to bring out [was] a form of altering the record [as in], let’s say, an

assassination—let's say the Zapruder film of [President] Kennedy's assassination, whatever number of seconds it took. Let's say thirty. Would you stretch that out to sixty [for an effect]?

KB: Of course! Just as a great scholar of literature would parse a sentence of Shakespeare. That's all it is. In one hand—in the hands of Oliver Stone [*JFK*, 1992]—that stretching out might be a manipulation that we would all find reprehensible because it had been folded and mixed in with conjecture and rumor and outright lie, along with a modest sprinkling of fact, and we would rebel against that *individual instance* of manipulation. But we would not dare question a scholar who would stop Shakespeare's play, *stop* the scene, *stop* the line, and parse it for its poetic content so that we might have more understanding of it. And I would submit that the slowing down of a piece of footage of veterans reuniting at the Civil War is a legitimate historical device if it does not in any way interfere with its fact. It is, in fact, the slowing down of the Zapruder film which gave the Warren Commission access to the truth of the event. It is in the [nature] of [such analysis we may place] Professor [Garry] Wills's magnificent dissection of the Gettysburg Address—I mean, it's just 272 words. My goodness! Let's just let it run! But he [Wills] gave us a dimension and a context to it. So it's really in what hands that kind of slowing down takes place.

TC: This is entirely beside the point, but . . . the Zapruder film has been chosen by the Library of Congress's commission [for preservation]—

KB: I saw that. That was so interesting.

TC: . . . I wanted to go through four or five films that I wanted to know whether you've ever seen [that have a place in documentary film history] . . . [For example], the *Yale Chronicles of America* series [in the 1920s].

KB: I haven't seen it.

TC: Okay. Pare Lorentz's *River* [1937] and *The Plow That Broke the Plains* [1936]?

KB: Absolutely. I have seen them.

TC: And World War II propaganda films, like *The Memphis Belle*?

KB: Seen many. Oh, you mean the Hollywood things.

TC: Not the current *Memphis Belle* but the one that won an Oscar in 1944.

KB: Yes.

TC: Let's see, what else? Television news in general in the 1960s? That's part of your—

KB: My consciousness? Very much so.

TC: And then Frederick Wiseman's films?

KB: Many. Most of his work.

TC: I wanted to ask you about them, not in any particular order but to see what effect they had on you as far as teaching you something or just your general psyche—

KB: Of all of the examples that you cited, I would have to say that it's the films of Pare Lorentz that would have had the most effect. I find them sort of "overripe." I found them initially overripe, but I found a kind of manipulation of film artistically, poetically, to be in my vein. I see terrific limitations in the *cinéma vérité* "truth" (and I put quotes around that) of Frederick Wiseman. I find television news, particularly today, almost shameful. It is not, in any way, living up to the promise of the medium or the promise of objectivity or the promise of—

TC: Or even the medium in the 1960s, when I thought it was at its height, covering the civil rights movement.

KB: I'm very impressed with what was able to be done with the relatively portable equipment; and, at that point, one had a sense that events were bigger than the form that was covering them. Now I believe that the form itself has subsumed all, so that reporters are bigger than the president. They know that they're permanent; there's no term limitations [on them], and so that kind of manipulation, I think, will kill our republic eventually, if there's not some self-governing mechanism that the press takes on. And that would include written as well as electronic news journalism.

TC: It seems difficult to anticipate a change like that, in that the First Amendment intrudes.

KB: I would not alter the First Amendment, but I would suggest that we were a much more secure republic not knowing [for example] that Franklin Roosevelt had a great difficulty standing up. Whereas today, knowing everything, we know nothing. That when everything becomes significant, there is *no* significance. When the medium becomes the message, as it is now, then a reporter is bigger than the subject that he's covering. And I don't know what will help us. I actually think this subject is a huge one to talk about; we could go for days on it. It is probably the single greatest threat to the republic, because it undermines our understanding of our history, it undermines the complexity of current events, and it really denies us the possibility of a foreseeable future as it—pretending to hide behind the First Amendment—essentially sells soap.

TC: Yeah. That's a hard question . . . not one that should concern us now, but it's terrifying to me that—

KB: The Gettysburg Address would not be covered today. Brit Hume [the White House correspondent] from ABC News would stand up underneath a tree as the president spoke behind him. He would say, “The president came to this tragic town to try to distract attention from his disastrous military campaign in the West”—meaning Chattanooga and Atlanta—and then would go on from there. C-SPAN wouldn’t cover it, it would probably be too short. They might have all of Edward Everett’s speech at the thing [Gettysburg], and [then] there might be that short thing. But even if it *was* covered, lost on C-SPAN as it was, filled in in an electronic environment where we are barraged. I mean, the words mean everything today, and they mean nothing. The words have become devalued. A picture is no longer worth a thousand words, it’s maybe 500 or 250, and words themselves have lost their importance because of the sheer volume of their ever-increasing brethren. And they’re diminished by the ancient animosities that set each group . . . against another, or one constituency against another. And so, all of a sudden, the . . . common consensus is frayed. And so [the] Gettysburg Address, the greatest single speech in American history, would be *lost*.

TC: A modern literary critic would refer to that as “postmodernism.”

KB: That’s exactly—it is one of the great tragedies. That is why there are many filmmakers much better than Pare Lorentz today who will never have the historical significance of Pare Lorentz, merely because of his placement in the pantheon. Just as there are better documentary filmmakers than Robert Flaherty, but because of his position [in documentary history]—he was the first to offer a certain sensitivity to certain things.

TC: Speaking of that position, I meant to put Flaherty and Lorentz in a continuum.

KB: I think they are both terrific and they are both artists.

TC: Well, their, in a sense, [contemporaries] were kept in a diminished state because they could not be advocates the way those two were allowed to be, particularly Lorentz. But in World War II, there was a breaking out of that [mode of “objectivity”], partly in order to make propaganda against foreign enemies. In any way, did the World War II films affect you the way Lorentz’s films did?

KB: Yes, and maybe even more than Lorentz’s; less than Flaherty’s, because Flaherty is the true artistic genius here, though many limitations and problems [shooting in the Arctic] as we now discover in the setting up and the re-creations and things that tarnish that [aesthetic reputation]. But there is terrific emotional force to that [World War II] propaganda. It’s not a kind of manipulation that I prefer, but *The Battle of Britain* [1943], for example, is incredibly potent film, and it’s wonderful. You know, you have to, even in this setting today, wonder whether D-Day could happen—I don’t think it could—I think there would be American reporters telling when it was going off, and that’s treasonous. You know, what,

finally, do you have a First Amendment for, if you can't defend the thing? I mean, the advocacy, the adversarial stance that the press has assumed in the years since Watergate has become only that, and they have forgotten to know what they're for. So there's this bland attempt at objectivity [that] has ensured mediocrity. The ferreting out of comic and simplistic villains has ensured that we have no heroes. Both of those are terrifically dangerous in a historical sense to a republic as fragile and as strong as us.

TC: Well, that seems to be accompanied by the decline of the parties, too. As though to say, the loyal opposition—

KB: Sure. The decline of the parties *followed* the press. The medium is the message. When the press shoots like the old Mexican bandits of those terrible B films at the feet of the gringos and says "Dance!" you dance. Unfortunately, we have a media that is so sanctimonious that it turns back over its shoulder to us and says, "Don't you hate the way they dance? Let's throw the bums out." And they have created the very situation in which the parties have diminished, because any freshman senator realizes he can appeal directly to the largest constituency by allying himself with an egomaniacal reporter, who should be the *last* person. And our talk shows are now filled only with reporters. We've even crowded out the politicians. They're the only people who are the perpetual talking heads now. It's a very sick and dangerous situation. I mean, the ordinary citizen, or the scholar, or the poet, who might be able to influence things, doesn't even *have* a voice, so you see the same faces over and over again, saying the same things over and over again.

TC: That's a very narrow market.

KB: It's a completely narrow market.

TC: You said a sentence [before] about Frederick Wiseman. I was just wondering: you would not put him in the same continuum [as having] an effect on you? That is, Lorentz meant more to you than he did?

KB: Yeah, because I think that I, some of Fred's films are incredibly important films, but I think he's off on an evolutionary trunk that has no future. That is to say, the strict adherence to this creed of *cinéma vérité* limits as much as it reveals. That the supposed unmanipulated truth that a *cinéma vérité* presentation has is just as manipulative, is just as refined, as anything that I do. It's just arrived at it in a different process. And if he doesn't see that, then he's missing the point. If I choose to point the camera in *this* direction, I have excluded everything else. If I choose to *use* this to the exclusion of seventy other hours in this film thing, I have left all of that out, and that is as devastating a selective process as my choice of this photograph of Lincoln or that photograph, this first-person quote, this talking head, this sound effect, this bit of music, this written narration. So I think that there is a kind of inflexibility, ultimately, to that process, which is why I believe the

cumulative cultural effect of his work has diminished in the last twenty years, where[as] he had a potent, potent statement to make in the 1960s and early 1970s.

TC: But would have had to adapt or evolve in order to keep that—?

KB: Yeah, and I've tried to talk to him about that. I mean, I still respect his work, and I have the utmost respect for him as a filmmaker and a craftsman, and I avidly await each new film, but I do believe that there is a kind of sense that that is a *truth*. [Jean-Luc] Godard said that film is "truth 24 times a second," and he was right. It's also lying 24 times a second, and that's *me*. You can quote me! [Laughs] I think that we have to remember that it's *all* selection. Just as the scholar, when he writes *this* sentence, he has not written a hundred million *other* sentences. And that is a limitation. And so there is no objective filmmaking.

TC: . . . *Roots* [1977] had an impact in its day similar to what your work has had in its day. But, considering your, what seem like extremely personal, thoughts and feelings about history, you could not do a *Roots*.

KB: I could do a dramatic film if [pause] Well, I don't know. I'm looking into the possibility of a dramatic film to express some of the things I feel, but *Roots* is different. *Roots* [is] apples, and I'm oranges. And I think it's easy and facile to lump them together as broad cultural effects. And they did have the same thing—they awakened people to history, and it makes people feel good, and that sort of thing. However, they're so totally different, and I know so little about how you make feature films. *Roots* was not intended . . . , it's an industrial product. It was intended to do some things that did not necessarily reflect the feelings of one particular artist. *The Civil War* was, and it drew on the talents of many artists and many scholars but [was], essentially, filtered through *this* person, and came out, and is, I still think, the best thing on the Civil War in *film*. But it is most definitely, you are correct to say, personal and artistic. And I believe, if I made a feature film, I could carry that artistic vision to it, but not having known the particular circumstances of *Roots*, I can't say . . .

TC: It seemed that it was taken seriously in its day as history, and in fact Alex Haley's book won a special Pulitzer citation as non-fiction—not as a novel.

KB: Yes, yes. That's exciting! You know, there are real, there are levels of inquiry, and we have to celebrate those that bring us to the door of the next level. And I think that *Roots* brings in a huge audience. Maybe *Civil War* has a little bit more select audience that brings you to something popular, like a *Roots* book, or somebody's biography that's a best seller. And then that brings you to something else, and something else. But all of it is enriching the academy as well as the populace.

TC: I'd like to go back. This is a follow-up. You were talking about how the academy . . . and I don't mean to go after them—

KB: Keep bashing?

TC: No, but, rather, almost in a technical way, what is it that must be left out of a film that would displease a historian? . . .

KB: I'm not sure there are any rules about what *should* be left out. I just think that by the time a film has finished its course, many things get cut out that disturb a scholar, who would like to see the digressions, who would like to see much more detail. There is a sense that no matter how many times in our film Wendell Phillips does say that Lincoln's a first-rate second-rate man, and we . . . as filmmakers excoriate [Lincoln] for his tardiness on emancipation, it's not enough . . . [Yet] I don't think it's wrong to be drawn to Abraham Lincoln and to be pained at his death. So [film] does deaths and assassinations better than it does the splitting hairs of tardiness, I suppose. But I say there are no rules . . . I'd love to go back. If I could go back right now and fill in things that I wished I could [have done]. I mean, remember also that scenes are cut from films *not* because you didn't want to consider this [or that] but because they didn't work visually or that there were too many notes. So, we had a huge amount of stuff in our original things on the political intrigue with the Black Republicans and [Congressman] Thaddeus Stevens and the relationship with Lincoln. And it was fabulous! And it *worked*. But it was just in Episode 2 or 3—it was too many; it was sinking our boat. And so we were willing to jettison some things. We also jettisoned other stuff [about which] you would say, "Oh, I'm *glad* you got rid of [it]."

TC: And "the boat" is finite in the sense that it is fifty-eight minutes, or whatever number of minutes you have?

KB: It's less *that*, than [that] it's finite in that it must *work*, it must flow . . . I work in public television so that nobody can tell me it has to be fifty-six minutes. They do tell all other filmmakers it has to be [thus], but I've said, "Look, it should take as long as it takes." There are people [who] say that a symphony is an hour and ten minutes long. [But] some symphonies are an hour and ten minutes long, some are an hour and a half, some are seventy-five minutes, some are fifty minutes, some are forty—I mean, it lasts as long as it lasts! . . . Like a sculptor, you bring this huge block of stone into the studio—and what is chipped *away* is as important to the final thing as what remains. It just has to, necessarily, be left on the floor of the studio or jettisoned outside of this boat. And that is true not only for a filmmaker but for the writer. The notes—the voluminous material—if you put them all in, no one would read it. It has to have some imposition of order. And I think the scholar is saying, "Yikes! If I apply my principles of order to a film, they've violated every rule, or most of the rules." And the enlightened scholar says, "This is a different set of rules." Just as you would not expect a play, or a dance, or a symphony to carry the same intellectual content as a historical [work] . . . You could do Lincoln and emancipation: you can do a film on Lincoln and emancipation, you can do an opera on Lincoln and emancipation, you can do a dance, you can do a symphony. All of those things, all have different rules that apply, they have common ground

between them, places where there is overlap. But you have to have—the academy, which is the bastion of intellectual freedom, has to have the freedom to tolerate other forms of expression, because the same principles work, there are just different sorts of rules . . . I mean, you read a chapter in something you write, and you cut something out as redundant or as something I can't handle in this particular chapter. Right?

TC: Yes. But it does seem that the slashing is most necessary in something visual. It's harder. [As a writer], *I* can leave it in, and someone will say, "Oh, it's [only] two more pages."

KB: That's correct.

TC: But it must come out of a film. As you say, the flow matters more than the length.

KB: Yes, the rhythm has to wait. You don't want your audience to fall out. Because you are ordering experience. Remember, [in] a book, you could read a paragraph and then set it aside and come back to it, read that again, [even] speed-read through the thing. You could give different [sorts of] attention. But I'm sequencing an experience, so necessarily I have to be attuned to a kind of very subtle inner barometer. I've just made a film eighteen and a half hours long. Nobody's ever made a film that long, right? It's a single film divided into nine chapters, but it's a single film. It's eighteen and a half hours long, and I've been roundly criticized for taking time. *I* think it's not long *enough*. To cover the things I wanted to cover in *Baseball*, I had to choose to do several players and teams to the exclusion of others, to many, many others. Just as in the Civil War, Wilson's Creek is a bigger battle than First Bull Run, but it wasn't mentioned in our film. There are many generals that weren't mentioned in our film. [And] there were all those other things that could have been mentioned . . . But, then, Robert Penn Warren wrote one of my favorite books about the Civil War, called *The Legacy of the Civil War* [1961], a most interesting—almost [a] pamphlet—of 125 pages. And Shelby Foote wrote an amazing one, and so did Eric Foner, and so did Barbara Fields, and Ira Berlin. You know, all of these are remarkable at different lengths. [Thus] the sort of specific demands of the project could produce a 125-page thing, a 3,000-page thing that required a lifetime of work, a huge documentary project of many thousands of pages, and other remarkable pieces of scholarly work that are all of different lengths. . . .

This is very exciting, to be able to organize this stuff. I mean, I think we are in a position where we want to be able to rescue history from itself without succumbing to all of the evils that come from thoughtlessness, speed, art, and other things. So it's a wonderfully terrifying razor's edge that we find ourselves on. And sometimes you make decisions that go over on one side or the other. But that's the kind of glory of art. I think the academy has for too long said it doesn't have to, it can't have an artistic aspect to it, or shouldn't, but its very best scholars have understood this. In the same way the greatest scientists or mathematicians or

physicists often find a spiritual dimension to their work. I asked C. Vann Woodward, if I was going to read *one* book on the Civil War, what would it be? That's the first question I asked him. He said *John Brown's Body*, by Stephen Vincent Benét [his epic poem, 1928]. But I would [in turn] say, if you really want to read a good book on history, you would read C. Vann Woodward, you know? *The Future of the Past* [1989], say, to begin with . . . He's one of the great historical stylists. He is so honorable and honest; I just worship at the feet of C. Vann Woodward. At the same time, I've been influenced by many, many other historians.

TC: That's as it should be.

KB: Yeah, and I want to be. In our body of consultants for *The Civil War*, it ranged from relatively conservative Confederate historians, you know, people who sort of love the Old South, to Berlin and Foner and Fields, to C. Vann Woodward and Robert Penn Warren and Shelby Foote—who knew every detail, knew where every body was buried in the Civil War—to others whose expertise was not specific to the Civil War, like Brinkley or Leuchtenburg, but who understood the process of historical filmmaking as well as the scholarly works and [who] could help bridge the gap. It was a really wonderful collection of people; some were popular historians, people who were outside the academy, like Bernard Weisberger, who used to be at Rochester but who, for the last fifteen or twenty years, has been a free-lance historian.

TC: Yes. He was at *American Heritage* . . .

KB: Well, no, he's never been *at Heritage* as much as he writes for *Heritage* a lot . . . He has just published a book on the La Follette family. And he's been terrific. I [prefer not to] go anywhere without Bernie, because he offers a kind of [*pause*] He's out there. He quit the academy for some of the very reasons that we're talking about—and said, "I still love history." He still applies the same scholarly methodology to his work[s], and sometimes they're successful and sometimes they're read by only a handful of people, but he's doing it outside of the academy and trying to appeal, I think, to a broad popular audience.

TC: Every once in a while, you express a kind of oneness with the academy, sometimes by picking out favorites such as C. Vann Woodward.

KB: Yeah.

TC: But you do speak of it as there being a gap.

KB: Yeah. I think that they've failed [in] the mission to educate the people of this country, and so I'm most angry about that. However, my work most resembles scholarly work. That is to say, my methods do not in *any* way appear the way other filmmakers work. It is, mine is, an archival, retrieval, research—huge research—job at every aspect. The hands-on, painstaking relationship to the evidence of the

past [requires] that I will spend five and a half years on *The Civil War*, . . . four and a half years on *Baseball*. Most closely resembles that. You know, where we collect material, we digest it, we think about it, we engage the services of scholars to advise us, we engage the services of historians to write. Remember, all of these are written by historians. Geoffrey Ward is a historian. One of the finest biographies I've ever read is his biography of Franklin Roosevelt, *A First Class Temperament* [1989]. And all along the way, we are really applying more academic and scholarly principles to how we work than filmmaking principles. We live outside of New York and Hollywood and in a small New England community more influenced by Dartmouth and Amherst and the five colleges and *this* city [Cambridge, Mass.] than any filmmaking center.

TC: Yes . . . I wanted to ask you one question about Shelby Foote. Why pick him to be the central voice in so many parts [of *The Civil War*]?

KB: I didn't pick him to be the central voice. He picked himself. The film picked him. We interviewed . . . many, many, many scholars, people, experts on the Civil War, most of whom—all of whom will remain nameless—could not give a sense of what *happened* during the Civil War . . . What Shelby had was both a rigorous intellectual relationship to the war but also an ability to place it in a narrative context. So he knew that if you asked him, "What happened here?" he didn't tell you how the war ended . . . You would ask a scholar, "Was it difficult for the Confederacy?" and Shelby would tell you that they ate "sloosh," which was a combination of bacon grease and corn meal, which they would cook into a kind of toast, wrapped around their bayonet, and that they [ate lots of] this. Someone else would say, "In 1862, things were difficult, but by '63 they were even bad, by '64 they had relented a little bit with the victories in the East, but by '65 and the surrender"—and you'd go, "Wait, wait, wait, just tell me *how* was it difficult?" So, finally, we would find ourselves putting into narration what the scholars told us—but told us in a way that couldn't be used. They never ended a sentence. I once shot an interview with a scholar who never landed the plane, never put a period at the end of what he said.

TC: [Laughs]

KB: And so I desperately, over an entire reel—twelve minutes—I finished, and I said, "Look, this is great—the information in it—but I can't use it because you're not *containing* it for me. I need to just have a simple idea—and I won't tell you what to say." We went three more reels. I never used a sentence. I tried desperately to edit things together, to try to bring something down. But if somebody leaves a sentence going up—because you've cut them in the middle—it doesn't sound like a sentence.

And so I had to go with someone who knew how to tell stories. I had no idea that . . . Shelby would emerge that way. We had many other people who we threw into the film, and we'd take them away. Because you go, "You've said that," or "I don't understand what that person said," or "Why did they tell me that the war ended?"

I don't want to know that." The great gift of history, the way you really can consider its influence, is to go back and *not think* that Lee is going to surrender this time. Because in that suspension of what you know to be the outcome, you participate with a variety of experiences, both intellectual and emotional, that make history, that *vivifies* history. People tell me, they write to me now, that they were sure after the sixth or seventh game of the Black Sox 1919 World Series that they were gonna play all right and, this time, that they would win! That's good history! Then you know the interior moments, as Whitman said, of that historical experience. So I did film . . . many, many, many scholars for this. And Barbara Fields [for example] is in, who's potent. Every one of hers [commentaries] is equal to ten or fifteen of Shelby's because it has such strong emotional content—and she's the scholar! She was much more emotional than Shelby was. Because she was saying this was about a *higher* principle—she was invoking Lincolnian language in her stuff.

TC: Yes. But it seemed to me that Foote's role was to be more of a presence, that is, a presence implying continuity, than she was. She was the analytic pause.

KB: Let's not—you see, there's where I think you're wrong. I think if you would go back, I would say that Shelby is anecdotal but more analytic. Barbara is absolutely—she—we used her as neutron bombs. She didn't actually have that many more bites—sound bites, as we call them—that were in, but she spoke for the filmmakers: that this was about a *higher* cause and that all the death meant nothing unless it would advance emancipation—as she says in one episode. So Shelby brings us along anecdotally, and he makes us *experience* the war the way the first-person diaries did. But Barbara spoke for the filmmakers, and it is such a pity that it has been reinterpreted. It is Barbara who is actually much more emotional in that film: the content of what she says brings with it the force of intellect, but she delivers it in an emotional package, where Shelby is anecdotal.

TC: It seems to me he's almost, because of his accent, speaking as though he were there. That is, anecdotal, as though a soldier were there.

KB: Well, he doesn't think he has an accent, he thinks *we* do, and it's very important to remember that. This is a broad American land that is, unfortunately, being homogenized, and I'm not sure there is an official accent in the country. However, Shelby speaks with the accent that many Southerners spoke—and speak—and he knows the war better than anybody else. Now, this is an interesting thing: when we had a big collection of scholars, we were, people were, arguing about things, but they would always turn back to Shelby. All of them. Because he *knew* what day the battle happened on, what the weather was like, what they wore, what they wrote, what they felt, why they were there, what the military stuff was, what the cause was. So that the scholar who had spent his life writing about causes had to defer to the man [who] could tell him not only about the causes but what the guy wore.

So not only, it's not so much his accent . . . I say, "The Gettysburg Address"—

that's all I had to say to him—and he says, “He [Lincoln] thought it was a failure. When [Lincoln] sat down”—Shelby cocks his head like this, and he tilts it—he said, “he [Lincoln] turns to Ward [Hill] Lamon and says, ‘That speech won’t scour.’ That’s what you say in the prairie when the dirt won’t come off a plow. He [Lincoln] thought he had failed.” But the tilt of the head makes you think Shelby’s *remembering*—he has read everything on it—and he is walking Lincoln down to [take his] seat. And so when he tilts his head, you think if this cameraman, Ken Burns, pulls [back], you’ll see Lincoln sitting next to Shelby and Ward Lamon on the other side of Lincoln. And you desperately want that—and *that’s* good history. And it doesn’t come because he knows how to tell a story. He *knows* how to tell a story, but he also read absolutely everything on that moment: the speech, what people wrote about it. “The cheek of every American must tingle with shame as he reads the silly, flat, dishwatery utterances of the man who has to be pointed out to intelligent foreigners as the president of the United States,” is what the *Chicago Times* wrote about that Gettysburg Address.

So [Shelby] knew what the apostasy was, he knew what Edward Everett said [on the same day], and so he walked us down [through it]. And that was his great gift. And so, finally, you trade for Babe Ruth, and you get rid of a lot of minor players . . . The guy covers all of the intellectual points but then also brings in art, humor, literature, feeling, affection. All of those things came with what Shelby said. And it was funny in the scholarly [consulting] meeting. Twenty-four scholars we had: C. Vann Woodward, Robert Penn Warren, Ira Berlin. [Yet] people at the end of this three-day conference were turning and saying, “But Shelby, what actually happened?” Because he *knew* what happened. With no notes whatever, he *knew* the day that the battle happened—not just that Antietam was significant, you know?

TC: He *knew* history?

KB: He *knew* history! He *felt* it and, in fact, perhaps *lived* it. And that’s the communication, and that’s the great mission. Is it enough just to be able to argue a point about the significance of Antietam that is slightly different from the previous generation’s “take” on Antietam and promote this as some gospel, when, in fact, more Americans [died], it’s the bloodiest day in American history? And what does that mean? “Well, yes, of course it is,” these others have told me, “of course it is, but you’re missing the point.” What is the point here? Of course this bloodiest day gave Lincoln the opportunity to do something he had been intending to do for a long time but hadn’t had the political—or, one would say, the moral—courage to do. All of which is in this film. But it is important to remember that Americans lost their lives here and to feel the texture and smoke of the battle. The best letter I’ve gotten on *Baseball* is from a guy who said, “I just sat down in front of my TV set for nine nights, close-up to the TV set, and I have dirt and tobacco juice on me.” I love the idea that my films might give off dirt and tobacco juice. And I will forever be looking for that, because I know that if he felt that, then he [absorbed] all the intellectual ideas about black emancipation and empowerment over the last century and a third since the Civil War, that all of the notions of heroism, of popular media and culture, of labor playing itself out, of

the rise and decay of great cities, all of the themes that quite consciously were woven into the *Baseball* film but never once didactically said, "And now we'll have a lesson about great cities and their decay."

TC: . . . Part of the theme of this interview is the change in the nature of historical truth over the last century, which saw a transformation from entirely print to the more mixed media of today. I wish you could have seen some footage from the *Yale Chronicles of America* series of the 1920s—a not quite halfway point in the century's journey.

KB: Well, . . . I think things have changed. But what we have the possibility, I think, is for a synthesis of the best of the old—what you called the "consensus"—and the new histories that are [from the] bottom up, or however you want to call them. And that strangely, this medium, which can be so correctly faulted for lacking the ability to contain very complicated intellectual ideas, is in fact the meeting place, I believe, where these strands can come together. That the kind of mystic chords that Lincoln talked about can sound within it. And I think it can be helpful to the academy, because if you're merely a polemicist or jealous or looking to score points, you miss the complexity of hearing a Southern voice next to a black voice, of hearing Morgan Freeman's exquisite voice [as] Frederick Douglass next to Jody Powell's [as] Stonewall Jackson. The fact that Powell [an aide to President Carter] is there is not some holdover, some Victorian drama that I am trying to do. I'm trying to confront these things and see what they do in collision with one another. And I think that there are powerful effects. Just as (often) politicians aren't as smart as the people, sometimes scholars aren't as smart as the people, too. The response to the series in ordinary letters is amazing. Thousands of people [were] awakened to complicated historical facts. You know, some woman said, "I see *The Civil War* [meaning my series] as an American Bhagavad-Gita, with its paradox of love and war, genius and ignorance, and the human transcendence that may one day show us where the real battles need to be fought—quite apart from geography. I look at the Civil War [the event] from a different perspective now." This is an ordinary woman in upstate New York. [Yet] not so ordinary. *That's* what you look for. *That's* what you live for!

To Turn as on a Pivot: Writing African Americans into a History of Overlapping Diasporas

EARL LEWIS

TWO AND A HALF DECADES after the appearance of the *American Historical Review's* inaugural issue, Benjamin Brawley authored *A Social History of the American Negro*, in which he wrote, "other races have come . . . but it is upon this one [blacks] that the country's history has turned as on a pivot."¹ The son of a leading post-Civil War black educator and clergyman, Brawley received one of the best educations available to men of his time—an undergraduate degree from Morehouse College in Atlanta, a graduate degree from Harvard University, and postgraduate training at the University of Chicago—and became a noted author, minister, and professor. By his death in 1939, he had written a dozen books, garnered the respect and admiration of members of the historical profession, and served as one of the first associate editors of the *Journal of Negro History*.² Thus it is all the more surprising that his interpretive history of African Americans went unnoticed by the *AHR*, the profession's premier journal.

Nor was this omission simply a matter of absolute neglect. Between 1895 and 1922, the journal published ten essays about blacks and more than a half-dozen reviews written by or about African Americans.³ While the majority of authors were white and committed to the evolving interpretation of slavery as benign and good, and Reconstruction as pernicious and ghastly, there was one notable exception. Nearly a decade and a half after the first issue, W. E. B. Du Bois, one of the

I would like to thank Elsa Barkley Brown, Frederick Cooper, Darlene Clark Hine, Robin D. G. Kelley, Marya McQuirter, Nancy Mirabal, Joe W. Trotter, Jr., and Hanes Walton for their help and suggestions. It is beyond the scope and intention of this article to cite everyone who has played a role in shaping African-American history. I would like to apologize to anyone who nonetheless feels slighted.

¹ Benjamin Brawley, *A Social History of the American Negro* (1921; rpt. edn., New York, 1970).

² August Meier and Elliott Rudwick, *Black History and the Historical Profession, 1915–1980* (Urbana, Ill., 1986), 5–7. According to Meier and Rudwick, as of 1914 only fourteen African Americans had earned doctorates in any field. Werner Sollors, Caldwell Titcomb, and Thomas A. Underwood, eds., *Blacks at Harvard: A Documentary History of African-American Experience at Harvard and Radcliffe* (New York, 1993), 4.

³ Included were essays by some of the leading figures in early slavery historiography. For a sampling, see Wilbur H. Siebert, "Light on the Underground Railroad," *AHR*, 1 (April 1896): 455–63; Ulrich B. Phillips, "The Origin and Growth of the Southern Black Belts," *AHR*, 11 (July 1906): 798–816; Edward S. Corwin, "The Dred Scott Decision, in the Light of Contemporary Legal Doctrines," *AHR*, 17 (October 1911): 52–69; N. W. Stephenson, "The Question of Arming the Slaves," *AHR*, 18 (January 1913): 295–303; Ulrich B. Phillips, "Slave Crime in Virginia," *AHR*, 20 (January 1915): 336–40; Marcus W. Jernegan, "Slavery and Conversion in the Colonies," *AHR*, 21 (April 1916): 504–27; Jernegan, "Slavery and the Beginnings of Industrialism in the American Colonies," *AHR*, 25 (January 1920): 220–40.

country's most learned men, became the first African American to have an article published in the *AHR*, and the only one for nearly seventy years. More surprising, in his 1910 essay, Du Bois asserted that Reconstruction was beneficial, a claim that threatened the emergent consensus that it was uniformly a "dark cloud" in the nation's history.⁴

Du Bois unquestionably benefited from a close relationship with his Harvard adviser, Albert Bushnell Hart, who was the president of the American Historical Association in 1909 and the *AHR*'s first secretary and treasurer. Hart maneuvered to have his star student address the Association's annual convention, and his efforts resulted in the publication of Du Bois's address as an article the next year. Yet, as Du Bois biographer David Levering Lewis solemnly noted, afterward the journal and the profession ignored Du Bois's interpretation for another generation. Moreover, the profession generally marginalized black historians, including the handful who were reviewed in the journal or who appeared as reviewers.⁵

Perhaps this is why Brawley's book received so little notice outside of African-American communities. Brawley was part of a pioneering generation of black intellectuals and social scientists who applied their training to the problems of their day. Even when ignored by white colleagues, these men and women would forcefully and directly challenge the accepted. Brawley, for example, envisioned his work as a corrective to those national tales devoted to the politically and economically influential. As he wrote, "it is necessary . . . to study the actual life of the Negro people in itself and in connection with that of the nation."⁶ During the very time that many scholars were inscribing blacks as objects rather than subjects of history, Brawley insisted that black life was well worth studying, given its inextricable link to the history of the nation. Or as he put it, "The race is not to be regarded simply as existent unto itself. The most casual glance at any such account as we have given emphasizes the importance of the Negro in the general history of the United States."⁷ More than four decades passed before anyone other than African Americans followed this lead.

With the explosive growth of social history since the 1960s, more and more historians have come to share Brawley's interest in the interlaced histories of

⁴ W. E. Burghardt Du Bois, "Reconstruction and Its Benefits," *AHR*, 15 (July 1910): 781–99. Although subsequent articles would appear by African Americanists, John Hope Franklin's AHA Presidential Address marked only the second appearance of an African-American author in the *AHR*: "Mirror for Americans: A Century of Reconstruction History," *AHR*, 85 (February 1980): 1–14. Stephen B. Weeks favorably evaluated W. E. B. Du Bois's book *The Suppression of the African Slave Trade* in January 1896: *AHR*, 2 (January 1896): 555–59.

⁵ On how the Du Bois–Hart relationship aided in the 1910 publication, see David Levering Lewis, *W. E. B. Du Bois: Biography of a Race, 1868–1919* (New York, 1994), 383–85; also, Meier and Rudwick, *Black History and the Historical Profession*, 5–6. For a discussion of Hart's early involvement with the journal, read J. Franklin Jameson, "The American Historical Review, 1895–1920," *AHR*, 26 (October 1920): 6.

⁶ Brawley, *Social History of the American Negro*, xxiii. For a review of the first generation of black social scientists, consult Meier and Rudwick, *Black History and the Historical Profession*, chaps. 1–2; Francille R. Wilson, "The Segregated Scholars: Black Labor Historians, 1895–1950" (Ph.D. dissertation, University of Pennsylvania, 1988); Anthony M. Platt, *E. Franklin Frazier Reconsidered* (New Brunswick, N.J., 1991); Kenneth Robert Janken, *Rayford W. Logan and the Dilemma of the African-American Intellectual* (Amherst, Mass., 1993); and Jacqueline Goggin, *Carter G. Woodson: A Life in Black History* (Baton Rouge, La., 1993).

⁷ Brawley, *Social History of the American Negro*, 373.

nationhood and African Americans. Today, a new perspective is taking shape, one with a clear indebtedness to Brawley and that first generation of professional historians who submitted black life to scholarly examination. It would be a mistake to call the emergent perspective a consensus; conflicting viewpoints remain a critical part of the enterprise. Still, despite the ostensible breakdown in comity and the perceived balkanization of the profession, patterns of convergence have appeared in the last quarter-century. This new perspective has three discernible features. Echoing Brawley, scholars acknowledge the central place of African Americans in the writing of the nation's history. They insist, however, that to study African Americans requires us to historicize the processes of racial formation and identity construction. Race in turn is viewed as historically contingent and relational, with full understanding of that process dependent on our abilities to see African Americans living and working in a world of overlapping diasporas (dispersed communities).

ALTHOUGH EXAMPLES OF THIS PATTERN are recent, they are part of a century of scholarship on African Americans. In its own way, the *AHR* played a pivotal role in shaping that scholarship. No topic figured more centrally than slavery, perhaps because no other issue seems so at odds with professed claims of liberty and justice for all. Slavery, after all, exposed a central paradox: a nation predicated on freedom nonetheless reconciled freedom and enslavement. Attention to this paradox also established African Americans as key actors in the unfolding of the nation.

Ironically, the study of slavery enslaved African-American historiography, at least in the pages of the *AHR*. Through 1945, scholars writing for the journal conflated slavery and blacks, viewing African Americans through the prism of the Peculiar Institution. Most of these early articles painted a picture of slavery heatedly disavowed later, but it was a portrait that the generation of scholars who lived through the Civil War or came of age during Reconstruction found quite appealing.⁸ To a certain degree, the story of slavery was not just about the bygone days of a "once upon a time" era; rather, slavery occupied a prominent place in the nation's memory because it framed the building of the Jim Crow era and situated the continuing contestation of power and race.

Given that most historians found themselves in lock-step with their time, it is not surprising that most founding members of the American Historical Association and its journal held fairly conventional views about race. African Americans may have been a part of the nation's history, but they were not the movers and shakers, nor were they perceived as the architects of their own destinies. The social inferiority of blacks was a given for most white historians; it shaped their work and figured in the conclusions they reached.⁹ Appropriately, the black historian Carter G. Woodson, founder of the *Journal of Negro History* and *Negro History*

⁸ Thus an article by Wilbur Siebert on the Underground Railroad, which appeared in the first volume of the journal, and whose very subject matter challenged the efficacy of the evolving argument, offered an early and fleeting example of a divergent perspective. Siebert, "Light on the Underground Railroad," 455–63.

⁹ Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, 1988), 74–78.

Week, author of numerous books on black life, and one among the group of black historians who challenged the prevailing perceptions of African Americans, wrote in a review of U. B. Phillips's history of slavery, "In just the same way as a writer of the history of New England in describing the fisheries of that section would have little to say about the species figuring conspicuously in that industry, so has the author treated the negro in his work."¹⁰ Woodson's critique notwithstanding, few who published essays or reviews in the *AHR* presented a more rounded view of black life.

Nonetheless, historians took more note of race and African Americans than did political scientists, for example. Writing in 1985, political scientist Ernest Wilson III observed that "political science, compared to the sister disciplines of sociology and history, has failed to generate sustained interest and scholarly breakthrough in the study of black life." Wilson attributed this failure to the "methodological orientations of the discipline."¹¹ But as Hanes Walton, Cheryl Miller, and Joseph McCormick have countered, history and political science began in roughly the same manner—coincident with the codification of segregation.¹² Unable to divorce themselves from larger social trends and practices, scholars in both disciplines adopted different approaches to the problematic of race, approaches that are reflected in the number and distribution of articles about African Americans.

During the years 1886 to 1990, the *Political Science Quarterly* and the *American Political Science Review* published twenty-seven articles each about African Americans. Comparatively, the *American Historical Review* printed a total of fifty-one substantive articles about blacks—twenty-nine essays on slavery, thirteen on African Americans, and nine on race relations—between 1895 and 1994. Just as important, sixteen of the twenty-nine articles on slavery had appeared in the *AHR* by 1945.¹³

These figures, however, mask significant clustering. Excluding the years 1896 to 1906, the *AHR* published at least one article per decade on slavery through the 1940s. After 1955, the number of articles increased in frequency, averaging one article approximately every 2.67 years for the period 1955–1970. This average disguises other patterns; for instance, the journal did not print an article on slavery, race relations, or African Americans between 1959 and 1965. Three of the six essays published during the period 1955–1970 were printed in 1970. Despite this progress, we are talking about one essay in every tenth issue of the journal.

Changes within the profession produced concomitant shifts in the coverage by the *AHR*. Between 1979 and 1994, the journal printed an essay on African Americans roughly every third issue, or a little better than one per volume.

¹⁰ Review of *American Negro Slavery*, in *Journal of Negro History*, 4 (January 1919): 103. Even the question of capitalizing "Negro" reflected differences in stature and status. As early as 1910, Du Bois debated Jameson over this matter in his essay: see Lewis, *W. E. B. Du Bois*, 385.

¹¹ Ernest J. Wilson III, "Why Political Scientists Don't Study Black Politics, But Historians and Sociologists Do," *PS: Political Science and Politics*, 18 (Summer 1985): 601.

¹² Hanes Walton, Jr., Cheryl M. Miller, and Joseph P. McCormick II, "Race and Political Science: The Dual Traditions of Race Relations Politics and African-American Politics," in John Dryzek, James Farr, and Stephen Leonard, eds., *Political Science and Its History: Research Programs and Political Traditions*, forthcoming.

¹³ Although it would be useful to know how many essays were submitted and rejected, what follows provides a general sense of what was published. Jacqueline Goggin discusses black participation in the

Equally important, this representation was more evenly distributed, with no more than three years passing before the *Review* published an essay in the field.

Nor was this pattern limited to historical journals. Even though they published approximately half as many articles as the *AHR* on the subject (twenty-seven each compared to fifty-one), political science journals also demonstrated a greater interest in race and African Americans after 1960, no doubt indicative of how the demands to desegregate penetrated the academy more generally. Fully 70 percent of the essays on race-relations politics and 60 percent on African-American politics published by the *American Political Science Review* appeared after 1960. Meanwhile, of the essays published in the *Political Science Quarterly* on race relations and blacks, 50 percent of the former and 63.6 percent of the latter came after 1960.

Among historians, even as black life received more frequent comment, the context for its exploration remained remarkably consistent. Slavery continued to dominate. In part, this focus reflected the dramatic increase in slavery historiography. No chapter in American history proved as needful of closer scrutiny as the Peculiar Institution. At the same time, the emergence of a spate of new journals in the 1960s, along with the continued importance of the *Journal of Negro History*, provided other outlets. Ironically, therefore, at the precise moment the *AHR* increased its coverage, devoting more attention to the history of black life, many of the more innovative developments in African-American history occurred outside its pages.¹⁴

NEVERTHELESS, given the means by which Africans first came to the Americas, the centrality of slavery is perhaps not surprising. And, on balance, the articles published in the *AHR* contributed mightily to new discussions in slave studies. Certainly, no single field of intellectual inquiry produced as radical a rewrite of

AHA and *AHR* in "Countering White Racist Scholarship: Carter G. Woodson and *The Journal of Negro History*," *Journal of Negro History*, 68 (Fall 1983): 355-75.

	<i>Political Science Quarterly</i> 1886-1990	<i>American Political Science Review</i> 1906-1990	<i>American Historical Review</i> 1896-1994
Topic			
Slavery	0	0	29
Race Relations	16	17	9
African Americans	11	10	13
Total	27	27	51

SOURCE: Hanes Walton, Jr., Cheryl M. Miller, and Joseph P. McCormick II, "Race and Political Science: The Dual Traditions of Race Relations Politics and African-American Politics," in John Dryzek, James Farr, and Stephen Leonard, eds., *Political Science and Its History: Research Programs and Political Traditions*, forthcoming.

¹⁴ A number of journals published important articles on aspects of black life, including the *William and Mary Quarterly*, the *Journal of American History*, the *Journal of Social History*, the *Journal of Urban History*, and the *Journal of Interdisciplinary History*. Excluding the years during which it halted publication, the *Journal of Negro History* continued to exceed all others in terms of output and range of coverage. Equally important, the period witnessed a dramatic increase in book-length studies, many of which were important and prize winning.

American history as the studies on slavery produced between 1950 and 1990. As Kenneth Stampp observed in the pages of this journal, a reexamination of slavery invited a new look at the institution and the practice of historians. Surveying the literature on the enslavement of African Americans, he complained in 1952 that "the literature dealing with southern Negro slavery reveals one fundamental problem that still remains unresolved. This is the problem of the biased historian." Seeking a middle ground between U. B. Phillips and Carter G. Woodson, and attempting to demonstrate race's irrelevance, Stampp would later coin his heatedly debated phrase "Negroes are . . . only white men with black skins, nothing more, nothing less."¹⁵ Implicitly, then, they lived as neither the childlike creatures manufactured by Phillips nor the complete designers of their own fates popularized by his few critics.

Stampp's choice phrase would eventually ignite a debate about race and identity. Ever since 1903, when Du Bois portrayed a divided soul as an alternative to either assimilation or separation, debates arose about the nature of black consciousness. Even if blacks had wanted to, Du Bois had written, they could not ignore their double consciousness. They occupied a particular place in American history, characterized by their "twoness—an American, a Negro; two souls; two thoughts; two unreconciled strivings; two warring ideals in one dark body."¹⁶ Yet, in his attempt to portray race as a construction—social and otherwise—and blacks as essentially the equals of whites, Stampp flattened Du Bois's poetically dense description. The twoness melded into a universal oneness, and questions of African-American identity retreated into the background. In fact, aside from his opening declaration, Stampp's pioneering account of slavery remained largely the story of a system of forced labor and exploitation.

Three years after Stampp's book appeared, Stanley E. Elkins returned us to the inner chambers of the slave's world. Interested in the work of Bruno Bettelheim in psychology, and a student of Richard Hofstadter, who a decade earlier had called for an interdisciplinary approach to the study of slavery, Elkins came to have an inordinate influence on the study of American slavery. His focus was more on arrested personality than group identity. Viewing slavery as a total institution akin to a concentration camp, Elkins advanced his now-famous Sambo thesis. Masters, according to Elkins, exerted tremendous control over their slaves. Allegedly stripped of a sustaining culture, deprived of social or psychic space, enslaved Africans supposedly internalized their subordinate status to such a degree that their behavior became infantile. His thesis sparked a historiographical

¹⁵ Kenneth M. Stampp, "The Historian and Southern Negro Slavery," *AHR*, 57 (April 1952): 613; Stampp, *The Peculiar Institution: Slavery in the Ante-Bellum South* (New York, 1956), vii. Before 1945, articles by Du Bois and one by Howard K. Beale, "On Rewriting Reconstruction History," *AHR*, 45 (July 1940): 807–27, portended a new direction in the study of slavery and emancipation. Du Bois's thesis that Reconstruction was beneficial and Beale's appeal that "we need studies of the Negro under Reconstruction . . . [not] colored by the racial prejudices of contemporaries who deemed even fundamental Negro civil rights and political activity unspeakable" awaited the arrival of a post-World War II generation of scholars who would refocus the nation's attention on what Thomas Jefferson labeled that "execrable commerce."

¹⁶ W. E. B. Du Bois, *The Souls of Black Folk* (1903; rpt. edn., New York, 1965), 215.

debate that lasted three decades, ultimately coalescing into a discussion of racialized group identity.¹⁷

Few paid much attention to Elkins at first, and when scholars took note they initially accepted his contention. Elkins's argument transferred the blame for slave behavior to whites, and some found this shift compelling, especially in the late 1950s and early 1960s. Yet the same social actors who at first accepted the argument began to question the role of resistance and autonomy in black life, twin themes that had a profound influence on post-1960 slave studies.¹⁸

Oddly, conspicuous silences emerged in the *AHR*. Between 1952 and 1970, the journal published just three articles on slavery. The first was Kenneth Stampp's aforementioned essay and the second a piece by Philip Detweiler on the Declaration of Independence and congressional debates over slavery. Although it would be another twenty years before the journal turned, in even the most cursory way, to the making of an African diaspora, it did print Carl Degler's comparative overview of slavery in Brazil and the United States.¹⁹ Neither of these articles—with the possible exception of Degler's—played a substantive role in the debates roiling slavery studies by the 1970s.

By the late 1960s, historians openly rejected the Elkins thesis. In its place, a more subtle portrayal of black life appeared. Implicitly, when not explicitly, historians asked how a people could reproduce so effectively if it had internalized its subordinate status as completely as Elkins suggested. Also, even though a number of historians would come to criticize its methodology and bold assertions, many teaching African-American history were forced to acknowledge for the first time Herbert Aptheker's important 1943 description of slave revolts.²⁰ How odd it seemed that the same system that produced Sambo could produce Nat Turner, Denmark Vessey, Gabriel Prosser, and many more. The incongruities forced a reexamination.

Contrary to August Meier and Elliott Rudwick's conclusion, it is not clear that an interpretive consensus developed from this reexamination, however.²¹ If anything, the colonial literature developed separately from the antebellum literature, with the latter coalescing around three approaches. Through the 1980s, most colonialists concentrated on explaining black life in distinct regions of British North America. As a result, we have pioneering studies of South Carolina, the Chesapeake region, the Middle Colonies, and New England. These historians not only avoided large macro studies, they also modified Elkins's and Stampp's

¹⁷ Meier and Rudwick, *Black History and the Historical Profession*, 241–47; Novick, *That Noble Dream*, 480–86; Richard Hofstadter, "U. B. Phillips and the Plantation Legend," *Journal of Negro History*, 29 (April 1944): 124. Ironically, Phillips had reached similar conclusions, stressing the congenital childishness of blacks and the paternalistic oversight of whites. Elkins attributed the end result to the victimizing nature of slavery.

¹⁸ Meier and Rudwick, *Black History and the Historical Profession*, 241–76.

¹⁹ Philip F. Detweiler, "Congressional Debate on Slavery and the Declaration of Independence, 1819–1821," *AHR*, 63 (April 1958): 598–616; and Carl N. Degler, "Slavery in Brazil and the United States: An Essay in Comparative History," *AHR*, 75 (April 1970): 1004–28. The journal did publish essays on other aspects of black life: Richard Bardolph, "The Distinguished Negro in America, 1770–1936," *AHR*, 60 (April 1955): 527–47; and Louis R. Harlan, "Booker T. Washington and the White Man's Burden," *AHR*, 71 (January 1966): 441–67.

²⁰ Herbert Aptheker, *American Negro Slave Revolts* (1943; rpt. edn., New York, 1987).

²¹ Meier and Rudwick, *Black History and the Historical Profession*, 274.

concern with personality and asked instead when Africans became African Americans.²²

In addition, colonial studies emphasized interaction between slaves and Europeans. Implicitly more than explicitly at this time, they began the discussion of African Americans in a world of overlapping diasporas—black, white, and red, initially. Taking clear note of demographic factors, staple production, spatial arrangements, and racial ideology, the literature covers several distinct zones of interaction. In South Carolina, for example, where newly arrived Africans and experienced slaves comprised the bulk of the population, a black majority developed. Whites preferred to live in the upcountry from the start, leaving Africans and their offspring a great degree of freedom. This majority introduced its own food products and horticultural approaches and created a creole language called Gullah to broker ethnic differences.

Historians who figured in the rewriting of the antebellum historiography painted their portraits of slavery on much larger canvases, favoring regional studies over local studies. One perspective, identified with cliometricians Robert Fogel and Stanley Engerman, equated slavery with other capitalist productions. Arguing that slaves had imbibed the Protestant work ethic and were more than the empty vessels alleged in the historiography, the two fashioned a story of African-American adaptation to a system of coerced labor. Although less convinced of slavery's overall damage to blacks than was Elkins, they nonetheless described a near total institution, an analysis that prompted a fierce and protracted debate.²³

Adopting a perspective the polar opposite of Elkins's is a group best called the "near total autonomists." This group includes among others Sterling Stuckey, George Rawick, John W. Blassingame, Leslie Howard Owens, Herbert G. Gutman, and Lawrence W. Levine.²⁴ Though cognizant of the brutalizing conditions of slavery, they found compelling evidence of a world beyond that created by the

²² See, among other works, Peter H. Wood, *Black Majority: Negroes in Colonial South Carolina from 1670 through the Stono Rebellion* (New York, 1974); Allan Kulikoff, "A 'Prolifick' People: Black Population Growth in the Chesapeake Colonies, 1770–1790," *Southern Studies*, 16 (Winter 1977): 391–428; Kulikoff, "The Origins of Afro-American Society in Tidewater Maryland and Virginia, 1700–1790," *William and Mary Quarterly*, 35 (April 1978): 226–59; Russell R. Menard, "The Maryland Slave Population, 1658 to 1730: A Demographic Profile of Blacks in Four Counties," *William and Mary Quarterly*, 32 (January 1975): 29–54; Edmund Morgan, *American Slavery, American Freedom* (New York, 1975). Through 1980, the standard source on blacks in colonial New England remained Lorenzo Greene, *The Negro in Colonial New England, 1620–1776* (New York, 1942); for an updated interpretation, see William D. Piersen, *Black Yankees: The Development of an Afro-American Subculture in Eighteenth-Century New England* (Amherst, Mass., 1988). Of course, influential overviews such as Winthrop D. Jordan, *White over Black: American Attitudes toward the Negro, 1550–1812* (Chapel Hill, N.C., 1968), did appear.

²³ Robert W. Fogel and Stanley L. Engerman, *Time on the Cross: The Economics of American Negro Slavery* (Boston, 1974). A hint of the controversy stirred by this book can be found in Herbert G. Gutman, "The World Two Cliometricians Made: A Review Essay of $F + E = T/C$," *Journal of Negro History*, 60 (January 1975): 53–227; and Paul A. David, et al., *Reckoning with Slavery* (New York, 1976).

²⁴ There is always a danger of pigeonholing and erasing important differences between members—political, ideological, and personal. Suffice it to say that each author paid close attention to black autonomy, while placing different emphases on the damaging effects of slavery. Sterling Stuckey, "Through the Prism of Folklore: The Black Ethos in Slavery," *The Massachusetts Review*, 9 (Summer 1968): 417–37; George P. Rawick, *From Sundown to Sunup: The Making of a Black Community* (Westport, Conn., 1972); John W. Blassingame, *The Slave Community: Plantation Life in the Antebellum South* (New York, 1972); Leslie Howard Owens, *This Species of Property: Slave Life and Culture in the Old South* (New York, 1976); Herbert G. Gutman, *The Black Family in Slavery and Freedom, 1750–1925* (New York, 1976);

slave masters. From sundown to sunup, Rawick asserted, blacks carved out both psychic and social space. They laughed, sang, worshiped, made love, argued, plotted, and told stories. During these times, they forged a community and created a degree of autonomy. Careful not to exaggerate, the near total autonomists wanted to explain how a system that supposedly generated only Sambos also produced Nat Turners. The explanation pivoted on asking a series of interconnected questions about people-making, about the tools developed to do more than just survive slavery.

Mediating the economic interpretation and the cultural interpretation was Eugene Genovese's refashioning of Phillips's paternalism model. While rejecting Phillips's blatant racism, Genovese allowed for a world in which masters cared for the people they enslaved. Slavery, Genovese noted in several seminal volumes, was a class-based system of economic exploitation.²⁵ Masters extracted the labor of slaves for the purposes of achieving a surplus. Still, economic imperatives did not eliminate the formation of affective attachments. After all, the typical enslaved person lived on an estate with less than twenty-five bondspeople. The size of the holding dictated a kind of social and psychic intimacy. Aware of considerable variation in the lived experience, Genovese did not shrink from the attempt to synthesize the clear dialectical relationship between profit motive and human empathy. What emerged was a subtle, complex portrait of the plantation world as "cruel, unjust, exploitative" and yet a system that "bound two peoples together in bitter antagonism while creating an organic relationship so complex and ambivalent that neither could express the simplest human feelings without reference to the other."²⁶

In spite of the power of Genovese's prose, how do we understand the interstices of African-American life? It is the collective character of the antebellum literature that demands further attention at this point. Through 1980, few of the most visible authors asked critical questions about how the forging of an African-American identity connected with the making of an American nation.²⁷ Because blackness was a given, historians of the antebellum period produced detailed but one-dimensional sketches of black slaves. Too often, they were either Sambos, Nat Turners, economic actors, or active participants in a Marxian-Freudian psychodrama. No one interrogated the very idea of ideal types as developed in the four approaches.

The pre-1980 literature showed the need for a more careful exploration of the range of human genius. The juxtaposition of the four approaches reveals a people

and Lawrence W. Levine, *Black Culture and Black Consciousness: Afro-American Folk Thought from Slavery to Freedom* (New York, 1977).

²⁵ Eugene D. Genovese, *The Political Economy of Slavery: Studies in the Economy and Society of the Slave South* (New York, 1965); *The World the Slaveholders Made: Two Essays in Interpretation* (New York, 1969); and *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974).

²⁶ Genovese, *Roll, Jordan, Roll*, 3.

²⁷ Sterling Stuckey was an exception; his efforts culminated in *Slave Culture: Nationalist Theory and the Foundations of Black America* (New York, 1987). Later scholars would criticize his perspective as predicated on the false assumption of a biological "race": Thelma Wills Foote, "The Black Intellectual, Recent Curricular Reforms, and the Discourse of Collective Identity," *Radical History Review*, 56 (Spring 1993): 53–55.

who molded themselves into being through their relationships with whites, reds, and one another. Regulated by time, spatial, and relational variables crosscut by power, the spectrum of potentiality included blacks psychologically damaged by the institution of slavery and others who constantly plotted to end its cruelty; across this spectrum, some did internalize the Protestant work ethic, and others participated in an evolving drama framed by social relations informed by paternalism.

The historical record is dotted with the names of those who lived the varied experiences. They were the Harriet Tubmans who stole themselves to freedom and in turn stole others. They were men such as the mulatto William Ellison, who purchased his own freedom and then kept others in bondage, or Robert Wright, the child of a white plantation owner and his slave concubine (who became his wife without the benefit of law). Wright both owned others of African descent and, more daringly, married a white woman in Virginia when doing so was a crime. There were those who snitched on Gabriel Prosser, the skilled Virginia bondsman who planned a conspiracy to take over Richmond and force an end to slavery, and those who served as slave drivers.²⁸ Remarkably, if we arbitrarily freeze the time frame, we would find that some occupied more than one position—obsequious servant, calculating sycophant, loving relative, would-be liberator. That is the story of African-American consciousness left unaddressed by the antebellum literature. What were the historical forces and factors that compelled people to make calculating yet contradictory decisions? Just as important, how do we understand their choices as contradictory?

Ira Berlin, on the cusp of this new emphasis on identity formation, anticipated the need to understand the making of African Americans. In an important 1980 essay in the *AHR*, he demonstrated the difficulties and importance of asking when and how an African-American society evolved.²⁹ From Berlin's careful synthesis of several decades of scholarship emerged a more elegant understanding of race and community. Rather than one people bound by filial ties to an African homeland, Berlin presented several peoples produced out of the vortex of history. They were Twi, Asante, Fulani, Yoruba, and Ibo; men and women caught in a system of forced labor that sustained a new economic order. In the midst of the Middle Passage, words were shared, customs exchanged, and dreams of freedom planted. There and in the Americas, these ethnic peoples became, first, African and then African American—that is, they came to see and think of themselves, in relational terms, as members of a larger collective.

During the colonial period, skin color and the circumstances of history became the determinants of slave status. Recall that well into the seventeenth century

²⁸ Rosalyn Terborg-Penn, "Black Women in Resistance: A Cross-Cultural Perspective," in Gary Y. Okihiro, ed., *In Resistance: Studies in African, Caribbean, and Afro-American History* (Amherst, Mass., 1986), 202–04; Michael P. Johnson and James L. Roark, *Black Masters: A Free Family of Color in the Old South* (New York, 1984); Thomas E. Buckley, S.J., "Unfixing Race: Class, Power, and Identity in an Interracial Family," *Virginia Magazine of History and Biography*, 102 (July 1994): 349–80; Douglas R. Egerton, *Gabriel's Rebellion: The Virginia Slave Conspiracies of 1800 and 1802* (Chapel Hill, N.C., 1993); he notes that Prosser was the name of Gabriel's owner and not one Gabriel used, p. ix; and William L. Van Deburg, *The Slave Drivers: Black Agricultural Labor Supervisors in the Antebellum South* (New York, 1979).

²⁹ Ira Berlin, "Time, Space, and the Evolution of Afro-American Society in British Mainland North America," *AHR*, 85 (February 1980): 44–78.

indentureship rivaled slavery as a pervasive form of coerced labor. Many of the first Africans brought to mainland British North America probably lived as indentured servants. After the 1680s, a series of laws codified the equation between race and slavery. With an increasing demand for sugar and tobacco, shifts in European labor markets, and the decline in other labor sources, slavery replaced indentured servitude. Thereafter, from Massachusetts to South Carolina, slavery defined the difference between unfreedom and freedom.³⁰

Although they would be viewed as the same by those among them blinded by slavery's ubiquity, Africans in the Carolinas molded a culture and history somewhat different from the relatively small population that resided in New England. Each regional population staked its own claim to becoming African American. The ruptures of history combined with the forces of memory to create these African-American populations. For African Americans living in Middle Colonies such as New York or New England Colonies such as Connecticut, election days became critical events. On these days, African peoples held their own elections of governors or kings, mocking in the process the ways, rights, privileges, and pretensions of Europeans. Through these means, Africans came to exercise a civic right denied them otherwise. Because election-day ceremonies also drew attention to their place outside the normal affairs of the nation, this open denial of national citizenship proved to be a key rupture, exposing the social distance between most blacks and most whites. The ceremonies then functioned as an important marker of group memory-construction, as blacks reminded each other of the shared nature of their plight.

Among blacks in South Carolina, with its more regular introduction of Africans recently removed from the continent, other socio-cultural patterns developed. Free of appreciable white supervision, members of the colony's black population created a creole language. They could do so because they had a numerical superiority, and in fits and starts they dominated—for a time. That they differed from blacks in New York was certain; that they imagined membership in a world that pivoted on the racialized practice of slavery was undeniable.

Thus, although Berlin wrote of one "Afro-American Society," in actuality he described several. Since, from the start, the slave population consisted of many peoples, something had to mold those who lived in a world of overlapping diasporas into African Americans. Racial slavery became the connector, linking blacks to one another across different regional political economies, with greatly differing demographic profiles and opportunities for gaining freedom. The process was uneven, with some stubbornly refusing to accept the binary constructions that fixated on black and white. Yet because race is and was not only marked but learned, produced, and reproduced in the matrices of daily living, at certain levels of consciousness African Americans accepted what they would at other times reject. In that sense, African-American identity was forged in the crucible of slavery, as part of an interaction between the "imagined" and the "real." Without

³⁰ Russell R. Menard, "From Servants to Slaves," *Southern Studies*, 16 (1977): 355–90; David W. Galenson, *White Servitude in Colonial America: An Economic Analysis* (Cambridge, 1981); A. Leon Higginbotham, Jr., *In the Matter of Color: Race and the American Legal Process; The Colonial Period* (New York, 1978).

question, this identity was shaped by what Evelyn Brooks Higginbotham has called the “metalanguage” of race—that is, race’s discursive ability to transfigure, transmute, and transform meaning.³¹

The powerful hold of race on the nation’s history is only now finding voice in this journal. Emblematic is a recent forum, in which appeared an imaginative essay and a penetrating critique by Michael O’Malley and Nell Irvin Painter, respectively. O’Malley artfully sketched a connection between the national conversation about money (specie) and race (species) in nineteenth-century America. He concluded that many drew the analogy between the two to rationalize a structure of difference. But, as Painter insisted, slaves were in fact both property and human, specie and species. Any analysis that failed to situate this critical problematic ultimately failed to theorize the meanings of race.³² Theorizing, however, is but the first step; connecting theory to the practice of history remains among the profession’s greatest challenges.

A second generation of revisionists, while acknowledging the need to integrate approaches gleaned from other disciplines with historical analysis, has further advanced our understanding of the process of people-making. Architectural historians have reminded us of the importance of studying material culture but in its vernacular forms. Consequently, we have learned how Africans and Europeans, sharing a common physical space, openly borrowed from one another. Historians of the Atlantic working class have demonstrated the importance of looking at sites of mutuality such as the racially and ethnically integrated slave, merchant, and pirate ships that traversed the oceans and inner waterways and gave rise to an international class of workers. Others, such as Charles Joyner, have demonstrated the usefulness of anthropologic methods and folkloric techniques. But, as Robin D. G. Kelley cautioned in a response to Lawrence W. Levine, the folk must be de-essentialized, too.³³ To date, historians of slavery have done a better job of noting the reception of a cultural message than explaining the processes of knowledge and culture transmission.

Equally important, the new revisionists have asked new questions about old issues. Toward this end, Brenda Stevenson, for example, has invited us to take

³¹ In this section, I use and build on points made by Thomas C. Holt, “Marking: Race, Race-making, and the Writing of History,” *AHR*, 100 (February 1995): 1–20; Evelyn Brooks Higginbotham, “African-American Women’s History and the Metalanguage of Race,” *Signs*, 17 (Winter 1992): 251–74. A word of caution is advised, however; as Clarence E. Walker reminds us in his sometimes testy account, there is a real danger if we manufacture bold but romantic histories of black life: Walker, *Deromanticizing Black History* (Knoxville, Tenn., 1991).

³² Michael O’Malley, “Specie and Species: Race and the Money Question in Nineteenth-Century America,” *AHR*, 99 (April 1994): 369–95; and Nell Irvin Painter, “Thinking about the Languages of Money and Race: A Response to Michael O’Malley, ‘Specie and Species,’” *ibid.*, 396–404.

³³ John Michael Vlach, *Back of the Big House: The Architecture of Plantation Slavery* (Chapel Hill, N.C., 1993); Daniel C. Littlefield, *Rice and Slaves: Ethnicity and the Slave Trade in Colonial South Carolina* (Baton Rouge, La., 1981); Mechal Sobel, *The World They Made Together: Black and White Values in Eighteenth-Century Virginia* (Princeton, N.J., 1987); Marcus Rediker, *Between the Devil and the Deep Blue Sea: Merchant Seamen, Pirates, and the Anglo-American Maritime World, 1700–1750* (Cambridge, 1987); W. Jeffrey Bolster, “‘To Feel Like a Man’: Black Seamen in the Northern States,” *Journal of American History*, 76 (March 1990): 1173–99; Charles Joyner, *Down by the Riverside: A South Carolina Slave Community* (Urbana, Ill., 1984); Lawrence W. Levine, “The Folklore of Industrial Society: Popular Culture and Its Audiences,” *AHR*, 97 (December 1992): 1369–99; and Robin D. G. Kelley, “Notes on Deconstructing ‘The Folk,’” *ibid.*, 1400–08.

another look at the bold findings of Herbert G. Gutman and a generation of like-minded colleagues. Stability, she has written, was only one aspect of family life under slavery. Among the enslaved she studied in Virginia, a number had their lives torn apart by jealous mistresses, economically desperate planters, unscrupulous family trustees, or by their own commitment to realizing freedom.³⁴ Her approach did something else: it moved the conversation beyond the structural perspective of the 1970s and 1980s to ask what worked, when, and how well.

Or as Gwendolyn Midlo Hall emphasized in *Africans in Colonial Louisiana*, French and Spanish control of Louisiana combined with the highly Africanized population from mostly the Senegambian region of West Africa to make for a remarkably different slave experience. After all, slavery was disorienting; no matter where, all Africans introduced into the international system of forced labor underwent a form of social death. Nonetheless, cultural similarities, common languages and dialects, and the ability to retain African names made the slaves' Middle Passage a less devastating experience than others have asserted, for those coming to Louisiana. More important, shared traits became a bridge, uniting the African-born and creole populations. Together, they demonstrated an ability to adapt and borrow, key to the making of an Afro-Creole culture.³⁵

Similarly, others writing after 1980 reminded us that men and women experienced slavery differently, and those experiences often occurred in interaction with Europeans and Indians. Indeed, the nation had crafted a tripartite image of black women as either Mammy, Jezebel, or Sapphire—subservient and asexual, licentious and extraordinarily sexual, or manly and castrating, respectively—by the time of the Civil War. Black women worked as hard as black men, suffered the sting of the lash as painfully, but only the women could be molested with impunity by any man. Few slaves were ever charged with raping another slave; those who were fell into a legal void. One Mississippi judge reversed the conviction of a black man accused of raping a ten-year-old child, commenting that the state knew no such crime. Slaves were caught in that liminal status between person and property. When a Missouri woman named Celia tired of her master's rapacious behavior and murdered him, jurors in that state rejected her argument that a woman—including a black woman—had the right to protect her honor. Each of these stories, and the ones to which they are connected, opened for review the as-yet unsettled terrain of power, gender, race, and sexuality in American history.³⁶

³⁴ Brenda Stevenson, "Distress and Discord in Virginia Slave Families, 1830–1860," in Carol Bleser, ed., *In Joy and in Sorrow: Women, Family, and Marriage in the Victorian South, 1830–1900* (New York, 1991), 103–23.

³⁵ Gwendolyn Midlo Hall, *Africans in Colonial Louisiana: The Development of Afro-Creole Culture in the Eighteenth Century* (Baton Rouge, La., 1992), esp. chaps. 6, 7, and 9.

³⁶ For works that explicitly mention multiracial contact, see Ted Ownby, ed., *Black and White Cultural Interaction in the Antebellum South* (Jackson, Miss., 1993); Gary B. Nash, *Red, White, and Black: The Peoples of Early America* (Englewood Cliffs, N.J., 1974); James Axtell, *The Invasion Within: The Contest of Cultures in Colonial North America* (New York, 1985). Deborah Gray White, *Ar'n't I a Woman?: Female Slaves in the Plantation South* (New York, 1985); A. Leon Higginbotham, "Race, Sex, Education, and Missouri Jurisprudence: Shelley v. Kraemer in a Historical Perspective," *Washington University Law Quarterly*, 67 (1989): 684–85; and Melton A. McLaurin, *Celia, A Slave* (Athens, Ga., 1991).

HISTORIANS OF SLAVERY were not the only ones to puzzle over the meanings of community, culture, and identity. Concurrent with the explosion in slave studies, others detailed the period of emancipation and postemancipation, as well as the period of twentieth-century community formation. Here, the *AHR* played a minor role in shaping the central debates. Instead, a number of important monographs appeared in economic, social, and urban history. These monographs, aided by an occasional essay, shaped most disagreements.³⁷

For more than a decade, beginning in the late 1960s, many postemancipation economic and social historians tried to understand the meanings of freedom for blacks. Implicitly, when not explicitly, social historians wrestled with C. Vann Woodward's pioneering thesis in *The Strange Career of Jim Crow*. Informed by a liberal assumption about integration into the social fabric of a nation, Woodward argued that segregation came late in the nineteenth century. With the exception of Thomas C. Holt's *Black over White* and Peter Rachleff's *Black Labor in the South*, most studies of Reconstruction turned on the story of interracial relations. An intellectually vigorous debate emerged over the timing of the introduction of the Jim Crow laws, producing provocative claims by Howard Rabinowitz, for instance, that exclusion predated segregation and that the latter was innovative.³⁸

Economic historians asked an important but strategically narrower set of questions about the meanings of freedom. They sparred over the rationality of discrimination and the reasons for a sharecropping system. Each work had its own merits, but most meritorious was the larger effort to understand how African Americans made the adjustment from unfree to free laborers. Incidentally almost, the body of this work forced African Americanists to explain the tensions between the state's conservative and progressive impulses—for example, the creation of

³⁷ This is not the place to provide a bibliography. Pertinent articles include Jonathan M. Weiner, "Class Structure and Economic Development in the American South, 1865–1955," *AHR*, 84 (October 1979): 970–92, and responses by Robert Higgs and Harold Woodman, 993–1001. Slavery remained the most prominent theme well into the 1990s. The exceptions were articles by Jane Landers on a free black town in Spanish Florida, which foregrounded a growing literature on the making of an African diaspora; special reflections on Malcolm X as a powerful icon by Nell Irvin Painter and Gerald Horne; Rebecca J. Scott's nuanced and comparative examination of emancipation; and Michael O'Malley's provocative discussion of the language of race and money, which Nell Painter critiqued: Jane Landers, "Gracia Real de Santa Teresa de Mose: A Free Black Town in Spanish Colonial Florida," *AHR*, 95 (February 1990): 9–30; Nell Irvin Painter, "Malcolm X across the Genres," *AHR*, 98 (April 1993): 432–39; and Gerald Horne, "'Myth' and the Making of Malcolm X," *ibid.*, 440–50; Rebecca J. Scott, "Defining the Boundaries of Freedom in the World of Cane: Cuba, Brazil, and Louisiana after Emancipation," *AHR*, 99 (February 1994): 70–102; and O'Malley, "Specie and Species"; and Painter, "Thinking about the Languages of Money and Race."

³⁸ C. Vann Woodward, *The Strange Career of Jim Crow*, 3d rev. edn. (New York, 1974); Thomas C. Holt, *Black over White: Negro Political Leadership in South Carolina during Reconstruction* (Urbana, Ill., 1977); Peter J. Rachleff, *Black Labor in the South: Richmond, Virginia, 1865–1890* (Philadelphia, 1984); Howard N. Rabinowitz, *Race Relations in the Urban South, 1865–1890* (New York, 1978); and "More Than the Woodward Thesis: Assessing the Strange Career of Jim Crow," *Journal of American History*, 75 (December 1988): 842–68. Important social histories of Reconstruction are discussed in Franklin, "Mirror for Americans"; Armstead Robinson, "The Difference Freedom Made: The Emancipation of Afro-Americans," in Darlene Clark Hine, ed., *The State of Afro-American History: Past, Present, and Future* (Baton Rouge, La., 1986), 51–74; LaWanda Cox, "From Emancipation to Segregation: National Policy and Southern Blacks," in John B. Boles and Evelyn Thomas Nolen, eds., *Interpreting Southern History: Historiographical Essays in Honor of Sanford W. Higginbotham* (Baton Rouge, 1987), 199–253. In addition, see James D. Anderson, *The Education of Blacks in the South, 1860–1935* (Chapel Hill, N.C., 1988). Even among scholars of Reconstruction, the study of interracial relations has diminished as a focus over the last decade.

the Freedmen's Bureau, the rare confiscation of white-owned land for black use, and the decision to force free people to work for their former owners in some instances.³⁹

Rebecca J. Scott recently brought the *AHR* into the center of the discussions. In one of the few essays published since 1980 on postemancipation societies, Scott successfully outlined the boundaries of freedom in the world of cane. Her comparison of sugar production after emancipation in Cuba, Brazil, and Louisiana is a careful reminder of the interaction between state policies, local practices, social position, and production. Moreover, her comparison underscores the fluid and fixed nature of identity formation, especially the politics of identity—understood as the temporally, spatially, and relationally mediated foregrounding and backgrounding of aspects of the self. In this sense, unfettered by the prerogatives of a highly racialized world, white Republicans and black voters temporarily elected officials they believed would protect their interests. Their hold on power lapsed when a coalition of planters, workers, and businessmen restored white dominion. Nonetheless, black-white voting and labor alliances provide evidence of the complexity of racial construction and the degree to which people lived in interconnected worlds demarcated by race, class, color, and other factors—a world of overlapping diasporas.⁴⁰

Elsewhere in the profession, a historical turn prefigured the linguistic turn that was still to come. Recently, much attention has been paid to those discursive practices that shape meaning. In addition to the more nakedly obvious forms of coercive power, Carroll Smith-Rosenberg and others have encouraged historians to assess the language of daily behavior. One purpose was the interrogation of the familiar; the other, a systematic appraisal of what we do not hear. On this point, Pierre Bourdieu argued, “because any language that can command attention is an ‘authorized language’ . . . the things it designates are not simply expressed but also authorized and legitimated.” The result, to paraphrase Michel Foucault, was growing awareness of the need to locate the local.⁴¹

Without benefit of such theoretical entreaties, a pioneering generation of historians advanced the importance of local or community studies. They, too, tried to locate the local, even as they attempted to follow Brawley's advice and situate their stories in a national context. Thus against the canvas of burning inner

³⁹ For a sample of this literature, see Harold Woodman, “Sequel to Slavery: The New History Views the Postbellum South,” *Journal of Southern History*, 43 (November 1977): 523–54; and “Economic Reconstruction and the Rise of the New South, 1865–1900,” in Boles and Nolen, *Interpreting Southern History*, 254–307; Roger L. Ransom and Richard Sutch, *One Kind of Freedom: The Economic Consequences of Emancipation* (Cambridge, 1977); Robert Higgs, *Competition and Coercion: Blacks in the American Economy, 1865–1914* (Chicago, 1980); Gerald David Jaynes, *Branches without Roots: Genesis of the Black Working Class in the American South, 1862–1882* (New York, 1986); and Gavin Wright, *Old South, New South: Revolutions in the Southern Economy since the Civil War* (New York, 1986).

⁴⁰ Scott, “Defining the Boundaries of Freedom in the World of Cane,” 72–81.

⁴¹ Among other works, examine Carroll Smith-Rosenberg, *Disorderly Conduct: Visions of Gender in Victorian America* (New York, 1985); Joan W. Scott, “Gender: A Useful Category of Historical Analysis,” *AHR*, 91 (December 1986): 1053–75; Pierre Bourdieu, “Structures, Habitus, Power: Basis for a Theory of Symbolic Power,” and Michel Foucault, “Two Lectures,” in Nicholas B. Dirks, Geoff Eley, and Sherry B. Ortner, eds., *Culture/Power/History: A Reader in Contemporary Social Theory* (Princeton, N.J., 1994), 166 and 202–10, respectively.

cities, historians sought to pinpoint the origins of a black ghetto.⁴² Moreover, they sought to distinguish between the riotous communities of the 1960s and 1970s and the forming communities of the 1910s and 1920s.

As Joe Trotter, among others, has commented, ghetto studies relied heavily on a victimization model, the very model being rejected by slavery scholars. Yet Gilbert Osofsky, Allan H. Spear, and others felt a need to account for contemporary conditions. Spear acknowledged in *Black Chicago*: "By 1966, no domestic problem seemed more complex, more fraught with danger to the social fabric . . . than Negroes, frustrated by generations of oppression, and whites, battling what they conceived as a threat to their way of life. Chicago, with its seething ghetto . . . , had come to exemplify the urban racial conflicts of the 1960s."⁴³ Why, Spear wanted to know, would thousands of African Americans seek a dystopian world in such numbers, if what they really sought was a promised land?

In pursuit of an answer, ghetto studies considered the process of ghetto making. They talked about "before the ghetto," the "shaping of a ghetto," or the "making of a ghetto." Informed by the mass movement of African Americans from the rural South to urban centers in the North and Midwest, ghetto studies paralleled studies of the first and second periods of intense internal migration—1900–1920 and 1940–1960. More important, ghetto studies, while concerned with race relations, detailed the creation of geographically circumscribed residentiality. Given the institutional focus of most in this genre, race became a transhistorical category, with an undifferentiated white population and an all-important black elite. Reacting to the forces of oppression, scholars paid little attention to the food people ate, the music they made, the loves they enjoyed, or the strategies they plotted.

In response to this important first step, a post-1980s group questioned the meanings of community for black urban dwellers. They adopted different conceptual approaches to explain this history, but they agreed as much as they disagreed. Eschewing the singular form, these historians spoke of an African-American community that existed in concert and conflict with many overlapping communities. Such constructions consciously challenged perspectives that defined community as a constantly forming site of potential utopia. Instead, as Elsa Barkley Brown has recently argued, community involved agreement and disagreement, with participation rather than unity of perspective being the operative frame. Because people never lived in just one community, and because those communities could easily accommodate utopian and dystopian events, people, and belief systems, uncomplicated notions of community denied the ways people actually lived their lives.⁴⁴

⁴² Representative works include Gilbert Osofsky, *Harlem: The Making of a Ghetto; Negro New York, 1890–1930* (New York, 1966); Allan H. Spear, *Black Chicago: The Making of a Negro Ghetto, 1880–1920* (Chicago, 1967); and Kenneth L. Kusmer, *A Ghetto Takes Shape: Black Cleveland, 1870–1930* (Urbana, Ill., 1976). For an overview of the literature, see Kenneth L. Kusmer, "The Black Urban Experience," in Hine, *State of Afro-American History*, 91–122; Joe William Trotter, Jr., *Black Milwaukee: The Making of an Industrial Proletariat, 1915–1945* (Urbana, 1985), Appendix 7; and Trotter, ed., *The Great Migration in Historical Perspective: New Dimensions of Race, Class, and Gender* (Bloomington, Ind., 1991), 1–21. Both Kusmer and Trotter updated their assessments in essays in the *Journal of Urban History* (May 1995).

⁴³ Trotter, *Black Milwaukee*, Appendix 7; and Spear, *Black Chicago*, 223.

⁴⁴ Douglas Henry Daniels, *Pioneer Urbanites: A Social and Cultural History of Black San Francisco*

Dystopia must not be confused with pathology, however. For the better part of this century, social scientists and social commentators have confused the underside of urban life and group sociologies. We have read over and over again about the pathologies that encapsulated the life chances and experiences of blacks. Even as a fourth generation of African Americanists attacked such notions in the 1970s and after, the perceptions endured. Crime, disease, and delinquencies became signatures of black life, and a full history laced with irony was replaced by stillborn images of people somehow outside of history—one-dimensional, never changing, always doomed to repeat the same day over and over again.⁴⁵

By the 1980s, a concerted effort was under way to write African Americans into their own histories. Scholars used words such as self-determination, agency, and empowerment to name the variations of those lived experiences. But, as Thomas C. Holt cautioned, the time had passed for fanciful portrayals of truly romantic proportions. Hagiographic accounts, common during the first two generations of African-American historiography, ceased as historians shifted from a history of contribution to a history of analysis. However, implicit in Holt's warning was the reality that even the powerful histories of black self-determination had to be balanced against the limits placed on individual actions.⁴⁶ Agency has come to mean not only unbridled resistance but also slow, irregular patterns of advance and regress, each in a dialectic working toward a synthesis.

A few see in the unavoidable link between race and racism in American or African-American history an inclination to assert the primacy of race and derive monocausal explanations. Enthusiastic proponents of a social-constructionist perspective sometimes forget, however, that such argumentation has long suf-

(Philadelphia, 1980); Trotter, *Black Milwaukee*; George C. Wright, *Life behind a Veil: Blacks in Louisville, Kentucky, 1865–1930* (Baton Rouge, La., 1985); Neil Fligstein, *Going North: Migration of Blacks and Whites from the South, 1900–1950* (New York, 1981); Peter Gottlieb, *Making Their Own Way: Southern Blacks' Migration to Pittsburgh, 1916–1930* (Urbana, Ill., 1987); James R. Grossman, *Land of Hope: Chicago, Black Southerners, and the Great Migration* (Chicago, 1989); Earl Lewis, *In Their Own Interests: Race, Class, and Power in Twentieth-Century Norfolk, Virginia* (Berkeley, Calif., 1991); Richard W. Thomas, *Life for Us Is What We Make It: Building Black Community in Detroit, 1915–1945* (Bloomington, Ind., 1992); Albert S. Broussard, *Black San Francisco: The Struggle for Racial Equality in the West, 1900–1954* (Lawrence, Kan., 1993); James Oliver Horton, *Free People of Color: Inside the African American Community* (Washington, D.C., 1993); Quintard Taylor, *The Forging of a Black Community: Seattle's Central District, from 1870 through the Civil Rights Era* (Seattle, Wash., 1994); Robin D. G. Kelley, "We Are Not What We Seem": Rethinking Black Working-Class Opposition in the Jim Crow South," *Journal of American History*, 80 (June 1993): 75–112; Elsa Barkley Brown, "Negotiating and Transforming the Public Sphere: African American Political Life in the Transition from Slavery to Freedom," *Public Culture*, 7 (Fall 1994): 107–46. For a description of the utopian and dystopian images of black urban life, read Charles Scruggs, *Sweet Home: Invisible Cities in the Afro-American Novel* (Baltimore, Md., 1993). Notions of community appeared in the civil rights literature, too. See Steven F. Lawson, "Freedom Then, Freedom Now: The Historiography of the Civil Rights Movement," *AHR*, 96 (April 1991): 456–71.

⁴⁵ For a very useful discussion of this literature and its implications, see Michael B. Katz, ed., *The "Underclass" Debate: Views from History* (Princeton, N.J., 1993). For a sense of the generational changes in African-American historiography, read Meier and Rudwick, *Black History and the Historical Profession*; and John Hope Franklin, "On the Evolution of Scholarship in Afro-American History," in Hine, *State of Afro-American History*, 13–22.

⁴⁶ Thomas C. Holt, "Whither Now and Why?" in Hine, *State of Afro-American History*, 4–5. In addition to several scholars mentioned in footnote 44, see the work of V. P. Franklin, *Black Self-Determination: A Cultural History of the Faith of the Fathers* (Westport, Conn., 1984).

fused the African-American intellectual tradition.⁴⁷ It is the rare African Americanist who cannot appreciate the myriad ways in which race is constructed. They have lived in or studied communities in which the eye wars with the brain. Black communities, after all, have long included people who range in skin color from black to white. The more difficult job has been explaining the processes of people-making: How was it that the blond-haired Walter White of the NAACP was as “black” as the darker-hued actor Paul Robeson? Of course, the state played a role in prescribing who was or was not black, who could marry whom, who could be a citizen and why, and who was entitled to certain governmental protections. But notions about race and membership are implicated in an individual’s understanding of community or communities.

Instead of marginalizing the role of race and racism, it is more fruitful to examine the interaction between memory, race, and community formation. As the ghetto specialists established and other scholars continue to show, *de facto* and *de jure* segregation served to limit the residential options of African Americans. In so limiting, segregation also brought together men and women of varied backgrounds and prejudices. Some have fancied this period between 1890 and 1945 the “golden age” of the ghetto. While an attractive formulation, perhaps, it is also a fantasy; there has never been a “golden age” of the ghetto.⁴⁸ First, not all urban areas developed the expansive, contiguous “ghettos” typically associated with Chicago and New York. Second, residential patterns in many southern and western cities diverged from northern and midwestern patterns. Through World War II, blacks scattered across the urban landscape in such cities; even when well-defined residential pockets emerged, they were not ghettos any more than similar concentrations of whites were. And third, throughout the period 1890–1945, residential proximity blurred significant class, color, and other differences in the black community. Nonetheless, black communities did form, and historians have to explain how and why. Typically, the explanations have turned on the notion of race as a socially manufactured reality that held people of diverse yearnings, backgrounds, and perspectives together in a web of shared aspirations.

Because we are at a conceptual crossroads, the renewed emphasis on race as a social construction should signal the beginnings of a conversation, not its end. To date, historians of African Americans have spent considerably more energy delineating the processes of racial construction than they have the complexities of

⁴⁷ See, for instance, Eric Arnesen’s undifferentiated critique of studies of African-American laborers and his positioning of the unquestionably important contributions of Judith Stein and especially Barbara Fields; Arnesen, “‘Like Banquo’s Ghost, It Will Not Down’: The Race Question and the American Railroad Brotherhoods, 1880–1920,” *AHR*, 99 (December 1994): 1602–07. Missing from his list are the writings of Nathan Huggins or the pioneering contributions of social scientists such as W. E. B. Du Bois and Alain Locke and writers such as Charles Chesnutt and Nella Larsen. Each recognized race as a social construction and a material reality decades ago. Nathan Huggins, *Harlem Renaissance* (New York, 1971), chap. 6; W. E. B. Du Bois, *Darkwater: Voices from within the Veil* (New York, 1920), 29–30; Alain LeRoy Locke, *Race Contacts and Interracial Relations: Lectures on the Theory and Practice of Race*, Jeffrey C. Stewart, ed. (Washington, D.C., 1992); Charles W. Chesnutt, *The House behind the Cedars* (1900; rpt. edn., Athens, Ga., 1988); and Nella Larsen, *Passing* (1929; New Brunswick, N.J., 1986).

⁴⁸ The issue of memory and race is one of the dominant themes in a special two-volume history of African Americans in the *Journal of Urban History*, 21 (March and May 1995). For a critique of the “golden age” argument, see Mark J. Stern, “Poverty and Family Composition since 1940,” in Katz, “*Underclass*” Debate, 220–53.

identity formation. This may seem a curious claim, given the explosion of articles and monographs that manifest an interest in identity. Yet the most searching and useful essays have been about race and racial formation. Insofar as identity is considered, it is racial identity that authors discuss. But race is but one part of the self, and race is always relational.⁴⁹ Equally important, black Americans have lived in variegated communities, where class, color, religious, and other differences mattered. Therefore, how do we understand the relational matrix of identity formation? Essentially, how do we begin to understand historical actors as multipositional?

My use of multipositionality has its roots in the debate over poststructuralism and postmodernism, although my usage may be viewed as a comment on and evaluation of the typical application of the term “subject positions.”⁵⁰ In thinking through the complexities of identity formation, we must consider several factors. To begin with, the study of identities includes but is not limited to the study of race, class, and gender. It is the interactive construction of identity—as child, lover, spouse, and so on—that requires fuller explication. Moreover, the self is constructed in space and time; to understand multipositionality, we need to understand how spatial and temporal factors lead historical actors to foreground or background constitutive aspects of themselves. Consider as well that relational differentials in power maximize the likelihood that certain forms of the self dominate at certain times. For instance, what a historical actor may say or do in a beauty salon may not be the same thing he or she would say or do in a church meeting, corporate setting, or confrontation with the police. If we are to take race seriously, we must begin in earnest to theorize and historicize how racial identity informs individual identity and how identity formation in turn informs racial construction—in a sense, we must take the processes of community-building seriously. Few of us, after all, acknowledge the number of selves competing for recognition at any given moment. Appropriately, human consciousness and

⁴⁹ For examples, review Howard Winant and Michael Omi, *Racial Formation in the United States: From the 1960s to the 1980s* (London, 1986); Anthony Appiah, *In My Father's House: Africa in the Philosophy of Culture* (New York, 1992); E. San Juan, Jr., *Racial Formations/Critical Transformations: Articulations of Power in Ethnic and Racial Studies in the United States* (London, 1992); Herbert Hill and James E. Jones, Jr., eds., *Race in America: The Struggle for Equality* (Madison, Wis., 1993). Here, I am in debt to Frederick Cooper, who has encouraged me to substitute relationality for identity, arguing that identities are formed relationally. Although I generally agree, it is also important to note that, once they form their identities, most actors acknowledge a constellation of influences.

⁵⁰ In distinguishing between poststructuralism and postmodernism, Ben Agger has argued that “poststructuralism (Derrida, the French feminists) is a theory of knowledge and language, whereas postmodernism (Foucault, Barthes, Lyotard, Baudrillard) is a theory of society, culture, and history.” Postmodernists interrogate social relations from multiple perspectives; most reject the creation of grand narratives that attempt to explain the broad range of human condition. The reducible parts are therefore substituted for the universal. Poststructuralists, on the other hand, assert the importance of decoding the language, a language that is part of a contested terrain. The job becomes to reveal the concealed, contextualize the text; as a result, we are encouraged to embrace the universal aspects of the human condition because so much of our lives is contingent. Ben Agger, “Critical Theory, Poststructuralism, Postmodernism: The Sociological Relevance,” *Annual Review of Sociology*, 17 (1991): 112. See also Linda Alcoff, “Cultural Feminism versus Poststructuralism: The Identity Crisis in Feminist Theory,” *Signs*, 13 (Spring 1988): 420–21; Nicola Gavey, “Feminist Poststructuralism and Discourse Analysis,” *Psychology of Women Quarterly*, 13 (December 1989): 459–75, esp. 464–66; and bell hooks, *Yearning: Race, Gender, and Cultural Politics* (Boston, 1990), chap. 2, for a small sample of approaches.

identity formation is understood as a complex personal and social calculus in which people simultaneously add, subtract, multiply, and divide aspects of themselves in other than a predetermined manner.⁵¹

The tension between racial formation and identity construction is manifested in the literature, especially in those works that seek to take race and class into consideration. Some labor historians, for example, have steadfastly insisted on the importance of the point of production. Able to document important moments of interracial or bi-racial unionism, they come close to ignoring historical actors as multipositional. In such accounts, it is often simply a matter of race *or* class or race *and* class. Either way, the conceptual design assumes that aspects of individual identity are simply additive or subtractive. This way, what workers learn and experience at work can be discretely separated from what they experience and learn at home—in their households and communities. While each work makes a certain contribution, adding important detail and texture to African-American history, the point-of-production approach adds to the confusion between racial formation and identity construction, in part because it helps produce a history of black life from the outside in rather than the inside out.⁵²

This is not true of all labor history, or all history of race-making and labor. Recent writings on the makings of a white working class underscore the degree to which white (like black, brown, red, and yellow) is a relational concept, formed in the mix of American social relations. As Alexander Saxton had previously noted and David Roediger jarred us into understanding, European immigrants defined themselves in relation to blacks. So, although it would be the late nineteenth century before Massachusetts Senator Henry Cabot Lodge granted Irish immigrants honorary Anglo-Saxon status—meaning the status of native whites—they had begun to form an understanding of themselves as white through participation in the Draft Riots of 1863, minstrel shows, and slavery.⁵³ Just as important, this understanding did not occur only at work but outside of work, when “white” workers returned home to their communities and households. There, they

⁵¹ For an example of the use of multipositionality in historical writings, see Earl Lewis, “Invoking Concepts, Problematizing Identities: The Life of Charles N. Hunter and the Implication for the Study of Gender and Labor,” *Labor History*, 34 (Spring–Summer 1993): 292–308.

⁵² While an exemplary study in many ways, Eric Arnesen’s *Waterfront Workers of New Orleans: Race, Class, and Politics, 1863–1923* (New York, 1991) typifies the strengths and weaknesses of such an approach. In his study, for example, color differences in New Orleans’ black community had little bearing on intraracial or interracial class alliances. Yet John W. Blassingame, *Black New Orleans, 1860–1880* (Chicago, 1973), 153–57, sketched a slightly different picture. Acknowledging the power of race to connect blacks of all hues, he also noted social differences based on color. Since blacks lived in a world of overlapping diasporas, to ignore a sizable portion of their world is to write them out of a portion of history. To his credit, Arnesen has recently commented on the need to examine black workers both in their communities and at work: “Class Matters, Race Matters,” review of Michael Honey, *Southern Labor and Black Civil Rights* (Urbana, Ill., 1993), in *Radical History Review*, 60 (Fall 1994): 230–35.

⁵³ For an important survey of African-American labor history, see Joe William Trotter, Jr., “African-American Workers: New Directions in U.S. Labor Historiography,” *Labor History*, 35 (Fall 1994): 495–523. Alexander Saxton, *The Indispensable Enemy: Labor and the Anti-Chinese Movement in California* (Berkeley, Calif., 1971); and *The Rise and Fall of the White Republic: Class Politics and Mass Culture in Nineteenth-Century America* (London, 1990); David R. Roediger, *The Wages of Whiteness: Race and the Making of the American Working Class* (London, 1991); Eric Lott, *Love and Theft: Blackface Minstrelsy and the American Working Class* (New York, 1993).

created a sociology of relational difference predicated on whiteness that elevated them socially.

Admittedly lost in the community studies literature is an appreciation of white ethnic differences. Whites—workers, ethnics, neighbors—have a monolithic character; too frequently, they all look alike. In this regard, historical efforts to differentiate must be more fully captured. More important, African-American historians must consider the processes by which various European immigrants became white at different times.⁵⁴ The story of European immigrants “passing” for white is one of the little-explored chapters in late nineteenth and early twentieth-century U.S. history. The telling of this story has profound implications for the study and writing of African-American history.

More than anything, however, there is a need to broaden our understanding of what has been called the process of community-building. Here, greater attention must be paid to the substantive roles played by black women from slavery through the civil rights movement; they are often relegated to subordinate positions in African-American historiography. A number of pathbreaking books and essays have revealed the ways in which men and women lived together and apart. Initially posed in much the same way as Sojourner Truth’s probing lament “Ar’n’t I a woman,” the studies of feminist scholars have consistently reminded us that all blacks were not men and all women were not white. From this critique has emerged a searching discussion of the important difference gender makes.⁵⁵

The growth of black women’s history has produced an associated development in gender history, too. From an early tendency for gender to mark the site of sexual difference, it has evolved into an analytical tool for uncovering the meanings of race and community. Literary scholar Barbara Christian has reminded us: “people of color always theorized . . . And I am inclined to say that our theorizing . . . is often in narrative forms, in the stories we create, in riddles and proverbs, in the play with language.” As Elsa Barkley Brown has noted, consciousness, politics, and even theory cannot be found simply in “position papers.” Using businesswoman Maggie Lena Walker as an example, she claimed, “Her theory and her action are not distinct and separable parts of some whole: they are often synonymous and it is through her actions that we clearly hear her theory.” Brown insisted, moreover, that gender was part of a womanist consciousness, a perspective formed in relation to black men and to non-blacks.⁵⁶

For those who willingly and easily disregard gender’s hold on black lives and the

⁵⁴ For an extended elaboration of this theme, see Earl Lewis, “Race, the State, and Social Construction,” in Stanley I. Kutler, ed., *Encyclopedia of the United States in the Twentieth Century* (New York, forthcoming).

⁵⁵ On the process of community-building, see Thomas, *Life for Us Is What We Make It*. For a review of important works in African-American women’s history not heretofore mentioned, see Darlene Clark Hine, “Lifting the Veil, Shattering the Silences: Black Women’s History in Slavery and Freedom,” in Hine, *State of Afro-American History*, 223–49; the compendium of essays in Darlene Clark Hine, Elsa Barkley Brown, Tiffany R. L. Patterson, and Lillian Williams, eds., *Black Women in United States History*, 16 vols. (Brooklyn, N.Y., 1990); and the essays in Darlene Clark Hine, Wilma King, Linda Reed, eds., *“We Specialize in the Wholly Impossible”: A Reader in Black Women’s History* (Brooklyn, 1995).

⁵⁶ Barbara Christian, “The Race for Theory,” in Abdul R. JanMohamed and David Lloyd, eds., *The Nature and Context of Minority Discourse* (New York, 1990), 38; Elsa Barkley Brown, “Womanist Consciousness: Maggie Lena Walker and the Independent Order of St. Luke,” *Signs*, 14 (Spring 1989): 610–33, quote on 631.

black community, Darlene Clark Hine offered an important word of caution. Implicitly inviting us to probe the gendered lives of black men, she highlighted the power of silence. Speaking of a culture of dissemblance in which, behind the gloss of public activity, black women held inside the most private hurts, especially rape, Hine reminded us of the presence of power valences that crosscut black communities.⁵⁷ Class, color, religion, and gender amount to a partial list of the forces that situated black life in a series of overlapping diasporas and made the telling of that history at once complicated and incomplete, necessary and important.

QUESTIONS OF CONSCIOUSNESS, identity, and community will demand greater attention from African-Americanist historians, especially as scholars examine with even more care and imagination postemancipation and urbanization. It is in the nation's cities, after all, where African-descended immigrants encountered American blacks, creating in the encounter what has been called a transgeographical America. Hailing from San Juan, Havana, Vera Cruz, São Paulo, and Kingston, from impoverished hamlets and towns, from the rural and urban South, these children of the African diaspora became living reminders of the diversity in black life. Seldom, however, have we asked when and under what terms they became African American.

In acknowledging race's powerful hold on black life and the nation's history, we have failed to ask an intriguing and important set of interrelated questions. This disjuncture in the literature is even more curious, given the willingness to ask such questions about life during slavery. For that earlier period, we assume that Africans who were imported from the Senegambian region differed from those from Iboland or Angola, and each of these groups differed from a creole population living in the West Indies. But what of the Jamaican migrants such as Marcus Garvey and Cyril Briggs, who settled in New York? What was the process by which they and hundreds of others existed as both African American and Afro-West Indian? How do we begin to understand differences within black communities? How do we define and refine the practice of writing African peoples into a history of overlapping diasporas?

The challenge is more than theoretical; it goes to the limits of one's historical imagination. For the better part of a decade, scholars who have written about the "other" have done so in terms of stark contrasts. Few have discussed the process of "othering" so critical to community-building.⁵⁸ Some of this "othering" set recent southern migrants apart from their northern-born neighbors. At other times, sexual differences became *deviances*; and, though black, men and women became outcasts in their own communities. This embrace of "othering" sent men and women into groups of affiliation based on skin color, religious background, and class presumptions and pretensions. But how did it happen that the

⁵⁷ Darlene Clark Hine, *Hine Sight: Black Women and the Re-construction of American History* (Brooklyn, N.Y., 1994), 37–48.

⁵⁸ Henry Louis Gates, Jr., ed., *"Race," Writing, and Difference* (Chicago, 1986), helps situate race as a study of difference; so too does JanMohamed and Lloyd, *Nature and Context of Minority Discourse*.

“othering,” as pervasive as it was, did not destroy the bonds of community? A convenient answer is always race. But this very convenience requires us to take a closer look at what is assumed in the answer.

Consider Arthur Schomburg, the black bibliophile and curator. Schomburg’s efforts to collect, collate, and preserve the records of African involvement in world affairs made him known to many. Today, he is remembered for the incomparable collection bearing his name at the Harlem branch of the New York Public Library. Few mention that Arthur Schomburg came to the United States as Arturo Alfonso Schomburg. The child of a mother from the Virgin Islands and a father from Germany, the brown-complexioned Schomburg arrived in New York from Puerto Rico around 1891. With what Alain Locke generously called an impossible command of English, Schomburg found safe quarter and employment in New York City’s multi-hued Puerto Rican and Cuban communities. As his involvement in fraternal organizations deepened, he increasingly moved into the world of black Americans.⁵⁹ Or, as Bernardo Vega commented, “He later moved up to the neighborhood where North American Blacks lived, and there he stayed. This led quite a few Puerto Ricans . . . to think that he was trying to deny his distant homeland . . . But his interest in the history of the Negro, their African origins and contributions to American society, led him to identify closely with black people.”⁶⁰

The history of how Arturo became Arthur and yet remained Arturo is the challenge for the next generation of scholars. It will require that we combine even more sophisticated conceptions of identity formation with even more imaginative historical questions and methods, that we recognize the permeability of boundaries and the multipositional nature of most human actors. Questions of politics, education, democracy, and labor will command center stage at times; so, too, will questions heretofore unimagined. And, as it has for over one hundred years, the *AHR* will alternate between playing a central and a peripheral role in the evolution of the resulting debates. Conceivably, such efforts will show in even greater detail and subtlety how the nation’s history has pivoted on the history of African Americans.

⁵⁹ Elinor Des Verney Sinnette, *Arthur Alfonso Schomburg: Black Bibliophile and Curator* (Detroit, Mich., 1989), 7–38.

⁶⁰ Bernardo Vega, *Memoirs of Bernardo Vega: A Contribution to the History of the Puerto Rican Community in New York*, Juan Flores, trans. (New York, 1984), 195.

History Solo: Non-academic Historians

NICHOLAS LEMANN

IN THE EARLY YEARS, non-academic historians wrote quite often for the *American Historical Review*. Theodore Roosevelt appeared in the first issue with a review of Alexander Scott Withers's *Chronicles of Border Warfare; or, A History of the Settlement by the Whites of Northwestern Virginia and of the Indian Wars and Massacres in that Section of the State, with Reflections, Anecdotes &c.* He found it to be a rousing account of "the deeds done by the rough backwoodsmen of the Alleghenies and the Upper Ohio in the ceaseless warfare of the white man against the red," though marred by an overreliance on local legend instead of verifiable fact.¹ (A few issues of the *AHR* later, Roosevelt himself was chastised by Frederick Jackson Turner on similar grounds, because, in *The Winning of the West* [1889–96], "he finds it necessary to use his history as the text for a sermon to a stiff-necked generation" and does not "give more time, greater thoroughness of investigation . . . and more sobriety of judgment to his work."²)

What happened to the non-academics? Rather than requiring an elaborate investigation to uncover, the answer lies in plain sight on Volume 1, Number 1, page 1 of the *AHR*, where William M. Sloane, in his opening statement of purpose, excitedly discussed the new possibility of "a science of the humanities," which could revolutionize the writing of history. Toward the end of his essay, Sloane got around to calling for a "history that shall be alike scientific and artistic," but there is no mistaking which side his heart was on: the scientific, not the artistic. Although he evidently was, like Roosevelt, wholly wrong about what we would now regard as the great historical issues (he suggested that historians follow the genetic-determinist lead of Francis Galton and think of themselves as compiling "the record of a continuous race-life"), Sloane's idea of what approach historians should take does not seem dated at all. He wanted them to get beyond "mere political history" written with a popular audience in mind, in order to probe the underlying issues for the benefit of fellow professionals. This would lead to a breathtaking intellectual maturation for historians:

the consequences are simply revolutionary, scientific methods having . . . been introduced into a discipline heretofore venerated as the highest department of prose literature, to be sure, but esteemed by the great critics, and by mankind generally, as on the whole vague

¹ Theodore Roosevelt, *American Historical Review*, 1 (October 1895): 170–71.

² Frederick J. Turner, *AHR*, 2 (October 1896): 175, 176.

and imaginative, as being a picture of the writer's own mind rather than a presentation of facts in an external world.³

Interestingly enough, the early issues of the *AHR* do not seem particularly faithful to Sloane's dicta, being full of unfootnoted profiles of individual leaders. Over the long run, though, the connection seems obvious between a feeling that history should become a science and the imposition of the Ph.D. as a professional credential, the deemphasis of narrative writing skills, a focus on underlying themes rather than on people, and the replacement of a popular audience for historians' writing with a smaller, specialized one. All this has happened over the course of a century but especially, it seems to me, since the 1950s, when political historians, whose work was aimed partly at non-colleagues, still occupied the highest rung of prestige within the profession. Am I wrong in finding it inconceivable that a top-notch younger historian today would embark on a multi-volume "life and times" of an American president, along the lines of Arthur Schlesinger, Jr.'s *Age of Jackson*, or an overview of the "American character" such as V. L. Parrington's *Main Currents in American Thought*?⁴ It is entirely defensible for historians to concentrate on narrower-gauge, or more theoretical, or (as they may privately frame the issue) less schmaltzy topics, but when they do the non-academic audience for their work will almost inevitably diminish.

Non-academic historians, named by what they are not, derive their function from what academic historians do not do. The wealthy, patrician historian of the Henry Adams or Francis Parkman variety having long since disappeared as a type, non-academic historians have to support themselves by writing, since, lacking doctorates as they do, they are ineligible for employment as teachers. That means they have to write history that will attract a popular audience, and *that* means they operate under a strong, unstated pressure to produce exactly the kind of work that Sloane wanted the field to move away from: irresistibly readable narrative biographies of political leaders. Leading examples from recent years would be Robert Caro's work-in-progress on Lyndon Johnson; David McCullough's *Truman*; Richard Reeves's *President Kennedy*; David Halberstam's group portrait of the Vietnam War's architects, *The Best and the Brightest*; and Taylor Branch's biography of Martin Luther King, *Parting the Waters*.⁵ Some of these writers have competition from academy historians, but it seems fair to say that, for the most part, their subjects would not strike academic historians as being at the pinnacle of intellectual challenge or importance. In other words, what non-academic historians are doing is filling a void that was willingly, even eagerly, left by the academy.

Now is an exciting time to be a non-academic historian. Although it is nearly impossible to make a living solely by writing historical books, publishers are eager to publish them and are paying more for them than ever before. Once one's books

³ William M. Sloane, "History and Democracy," *AHR*, 1 (October 1895): 1, 7, 6, 2.

⁴ Arthur M. Schlesinger, Jr., *The Age of Jackson* (Boston, 1945); V. L. Parrington, *Main Currents in American Thought: An Interpretation of American Literature from the Beginnings to 1920*, 3 vols. (New York, 1927).

⁵ Robert A. Caro, *The Years of Lyndon Johnson*, 2 vols. (New York, 1982-); David McCullough, *Truman* (New York, 1993); Richard Reeves, *President Kennedy: Profile of Power* (New York, 1993); David Halberstam, *The Best and the Brightest* (New York, 1972); Taylor Branch, *Parting the Waters: America in the King Years, 1954-1963* (New York, 1988).

have become established, a *modus vivendi* involving lectures and magazine assignments can usually be cobbled together. There is the feeling of being part of a movement, a rising tide. The country seems to want these books and to respond to them; they become the springboard for wider debate; although this may be only a temporary aberration owing to the White House's currently being occupied by an avid reader of nonfiction, a few of the leading non-academic historians have found themselves brought in and asked for advice by the president of the United States. Academic historians have also treated us well—it is a pretty safe bet that no journalist is going to be asked to contribute to the centennial issue of the *American Sociological Review*.

Self-congratulation is not the job at hand here, though. I would like to do two other things: explain, for the record and to engender discussion, how non-academic historians work and, somewhat in the spirit of William Sloane a hundred years ago, propose an overall standard or mission to which non-academic historians might aspire in order to emphasize their strengths and diminish their weaknesses.

NON-ACADEMIC HISTORIANS tend to be shaky on historiography. Academic historians select their projects while steeped in the reading of history. They know what the important unresolved questions are. Non-academics are usually only barely aware of the academic work on a subject during the time that they are deciding what to write about. What is at the top of their minds is a desire to attract readers and, more generally, a journalistic instinct (most of us spent our early careers as reporters) for “the story”—not, as a primary purpose, to advance historical discourse. Thus two concerns are paramount in choosing a subject: that it have a narrative structure and that it be obviously relevant to the present day. Biography is therefore the most natural form for non-academic historians. A life has a built-in story line, and it is fairly easy to tell in advance which historical figures (especially presidents) are going to resonate today.

To someone trained as a journalist, the first impulse upon undertaking a biographical project is not to head for the archives but to begin interviewing: the subject, the subject's family, the inner circle, friends, rivals, enemies. These interviews have a *Citizen Kane* quality, in the sense that what is hoped for is that the subjects will, instead of just responding to questions, deliver vivid, perfectly formed anecdotes, because that is what makes the best narrative material. Good interviews for historical books are not conducted in the manner of televised journalistic interviews, with their interrogatory structure. The interview subject is supposed to feel relaxed to the point of speaking completely freely—not be on guard. One of the pleasures of doing interviewing of this kind, for a former newspaper reporter, is discovering how open, frank, and voluble powerful people often become within a few years of leaving power. The tight-lippedness of journalist-politician interchange inheres, one discovers, in the way big events unfold, not in the personalities of politicians.

I have felt some disapproval emanating from colleagues who are academic

historians when I discuss this kind of interviewing with them. They believe that the contemporary record is far more reliable than long-after-the-fact reminiscences by people who are, in effect, consciously playing to the historical jury. Among the materials in historical archives, it is my impression that oral history, based as it is on interviewing, is the second-class citizen. To my mind, however, the case against historical interviewing is unpersuasive. In all interview-based research (notably news reporting), it is essential, and fairly easy, to cross-check a subject's memories against other people's memories and the written record. Not only does it quickly become clear how fully a subject's recollections can be trusted, but no subject ever has to be trusted completely; the possibility of independent verification is always there. Even a subject who turns out to be completely untrustworthy, or cannot remember anything, is still worth interviewing. If as a writer you are going to have to create someone as a character on the page, it is invaluable to spend some time in that person's physical presence. Also, the contemporary record does not strike me as being dramatically more trustworthy than the interview, because it is a standard feature of organizational life that what is put down on paper does not make a perfect fit with what is actually happening.

On the other hand, it is true that non-academic historians as a group tend to be uncomfortable with the printed-matter side of historical research—both the reviews of the literature and archival work done with primary source material. In the latter case, human nature, to some extent, rebels against archival research. Certainly journalistic nature does. The long hours of drudgery, the travel requirements, the expense—all these make it a slow-payoff form of legwork. Mastering the historiography on a subject is much easier for academic historians because they will have already limited themselves to a sub-specialty during graduate school and become quite familiar with the literature on it, so all they have to do is keep up with new work as it is published. Non-academic historians are a little unused to reading as a form of research to begin with and do not begin each book with an existing base of knowledge. We tend to switch fields, in effect, with each book and so have to begin tackling the literature anew each time. In addition, we are likely to pick topics that cut across several academic specialties, difficult to master simultaneously. All I can suggest to help correct these problems is increased contact between academic and non-academic historians, with the goal of making the non-academics aware of what they need to read.

These are generalities about the way non-academic historians work. To make them more specific and therefore vivid, I would like to use myself as a case study.

I HAD THE GOOD FORTUNE to be born to parents who, while not professional academics, writers, or intellectuals, both read, wrote, thought, and discussed ideas constantly. I grew up in an atmosphere of reverence for, and genuine excitement about, the life of the mind. In addition, my parents—my father a conservative business lawyer, my mother a liberal child psychologist—disagreed about virtually every public question, which put me in the position of constantly having the healthy exercise of figuring out whom I thought was right.

Two things, it seems to me in retrospect, got me interested in history. First, I was born in New Orleans a few months after the Supreme Court's *Brown vs. Board of Education* decision was handed down, so I grew up with a large historical event going on all around me. The way in which it was going on is worth noting, too: I had almost no awareness of the public events connected with the civil rights movement, with the exception of the passage of the 1964 Civil Rights Act. I experienced historical change as something that works its way deeply into the fabric of ordinary life, rather than as something best understood in terms of public milestones. My whole childhood now looks to me like an ongoing private discussion by whites of black people and civil rights, over the course of which a distinct though by no means complete attitudinal change took place. This put it in my mind that it is at the invisible level that the working out of history is most interesting.

When I was an undergraduate at Harvard, I studied American History and Literature, a hybrid "committee on degrees" that in my day was dominated by the ghosts of three dead men, Perry Miller, F. O. Matthiessen, and Kenneth Murdock. This was my second formative intellectual experience. Although at the time the theory of History and Literature was out of academic fashion and used to be taught by graduate students with palpable embarrassment, it made a deep impression on me: one could learn about history by reading works of literature and by applying literary-analytical techniques to historical texts; and, the subliminal message, if you wanted to be a Real Writer, you could do so by writing history or some related form of nonfiction, rather than having to stick to fiction and poetry.

After college, I embarked on a career in magazine journalism—or, in a sense, re-embarked, since I had begun writing for a weekly alternative newspaper in New Orleans while still in high school. I worked for *The Washington Monthly*, a small political magazine, *Texas Monthly*, a big, glossy regional magazine, and the *Washington Post*, and finally wound up at *The Atlantic Monthly*, where I have been employed since 1983. All this time, I was to some extent involved in a project of self-training toward the goal of writing big, ambitious nonfiction books. What I was learning, as a journalist, was mainly how to identify, research, and write work in the narrative form. At some point in the process, I felt technically ready to write books, but there was an intellectual problem that held me up: the difficulty of applying nonfiction narrative skills, which naturally lend themselves to microcosmic work, to the kind of large social-historical themes that I wanted to explore.

Finally, I decided that I would never resolve the problem in the abstract, so I had better just plunge ahead and try somehow to work it out as I went along. That was the spirit in which I began my most recent book, *The Promised Land*. The theme was race relations, in particular inner-city ghettos, which I saw as being the unstated driving force in American politics, geography, and culture.⁶ But what would the story be?

Somewhat as an excuse to begin spending time in an inner city, I undertook a project on a welfare mother in Philadelphia, Mary Manley. I began, like most

⁶ Nicholas Lemann, *The Promised Land: The Great Black Migration and How It Changed America* (New York, 1991).

white outsiders to this subject, with the notion that the welfare system had created the ghettos, so viewing the ghettos through a welfare lens made sense to me. In addition to spending a lot of time with Mary Manley, I tried to familiarize myself as broadly as I could with her neighborhood, North Philadelphia. I interviewed politicians, experts, and community leaders; I tried to find local histories in libraries, and I spent a good deal of time just driving around the area, stopping in here and there. The main impression that emerged from several months of this was that welfare had been a minor factor in the creation of the ghettos; the major factor had been migration. Mary Manley was herself a migrant from the rural South, and, more broadly, it struck me that her neighborhood and others like it had undergone unusually rapid demographic changes over three decades, having changed from all white to all black, and then from crowded and mixed-class to depopulated and all-poor. This was the result of a series of migrations within the metropolitan area triggered by the mass migration of African Americans from the South during and after World War II. And migration is a subject that naturally lends itself to a narrative treatment.

I found that academic historians, oddly, had done a lot of good work on the first phase of the great black migration, during and after World War I (for example, James Grossman's excellent *Land of Hope*), but almost nothing on the second and much larger phase from 1940 to 1960.⁷ One reason for this may be that academic historians are not under as much institutional pressure to be topical as non-academics are, and another is that there is more archival material on the first phase of the migration. But my archive would be interviews with those who had migrated; that was a reason for me to concentrate on the second phase, because there would be so many more living participants.

I went to Chicago to restart the research, having picked it as a location because it seemed to be the richest setting for my subject. On an early trip there, I was listening to my car radio and heard an announcement of the upcoming thirtieth reunion of the Canton, Mississippi, high school class of 1955. A Mississippi high school reunion being advertised in Chicago! I jotted down the number given and called it, and so began a period of several months during which I was interviewing members of that class in Chicago and Canton. This was invaluable in familiarizing me with the subject, but when I had finished I decided, as I had after the Philadelphia project, to throw out all my research material and start over. I had two reasons. First, although I wanted to present a total picture of the migration, I remained committed to my original idea of explaining how the ghettos were created. Almost by definition, because they were high school graduates, the group from Canton had done well and did not live in the ghettos anymore: they belonged to the successful majority of black migrants. If I stuck with them as subjects, I would have to discuss the ghettos in generalities and could not portray daily life there. Second, the richness of Chicago in the North is echoed in Mississippi by the Delta, the cotton-growing section that was home to the sharecropper system and to the blues. Canton is in another part of the state. I decided it would be a mistake not to use a Delta location in Mississippi.

⁷ James R. Grossman, *Land of Hope: Chicago, Black Southerners, and the Great Migration* (Chicago, 1989).

So I returned to Mississippi, rented a car, and began location-scouting in the Delta. I settled on the town of Clarksdale and began to search for subjects there and in Chicago. I tried a variety of techniques, including searching church records and social-service agency file and canvassing neighborhoods. Two ways of searching worked best. Every town of any size in Mississippi has a club in Chicago; these clubs were an excellent source—but only for middle-class migrants, because poor people usually do not belong to them. For poor migrants, the best source was public housing in Clarksdale, virtually all of which is run by a single private developer, Bennie Gooden, who befriended me and instructed all his building managers to put me in touch with any tenants who had ever lived in Chicago or had relatives in Chicago. Of the main characters in the book, one, George Hicks, was then the president of the Clarksdalians Club of Chicago; two, Ruby Haynes and Uless Carter, were public-housing tenants in Clarksdale; and one, Constance Daniels, a public-housing tenant in Chicago, was a former daughter-in-law of Ruby Haynes.

I had a large remaining problem—which, again, I conceived of as a problem of narrative structure more than of analysis. Most books that deal with the subject of ghettos take the form of either policy analysis or close-in personal description. The latter is a narrative-friendly technique, the former is not. I wanted to find a way to include policy-analysis material in my book without losing the story line of my subjects' journey to the North. Reading Allen Matusow's *Unraveling of America* gave me the idea that I might use the history of President Johnson's War on Poverty as a narrative vehicle for the policy analysis.⁸

I then, for the first time in my journalistic career, did some primary-source archival research, mainly in the Kennedy, Johnson, and Nixon presidential libraries. With the exception of Matusow's book, which is a general history of the 1960s with a few chapters on the War on Poverty, this was, I found, like the latter phase of the Great Migration, a substantially untouched subject on which there was a wealth of material—and, as with the migration, most of the key participants were alive and available to be interviewed. The War on Poverty and the Great Migration fit together amazingly well: most of my migrant-subjects had been directly touched by the War on Poverty in some way, and my policymaker-subjects had had the migration constantly in mind as they did their work. Putting both stories under the same roof allowed me to stay in a storytelling mode almost all the time, and it gave the book a large-canvas feeling by allowing it to encompass locales ranging from the White House to remote shacks in the Delta.

FOR A VERY LONG TIME, I have been interested in the subject of success and ambition in America, because, along with race, it seems to me to be one of our defining national obsessions. I would have written a book about it before *The Promised Land*, except that the problem of figuring out what the narrative line would be seemed even more daunting than it did for the theme of race relations. The obvious way to structure a book about success in America would be to trace

⁸ Allen J. Matusow, *The Unraveling of America: A History of Liberalism in the 1960s* (New York, 1984).

the rise (and possibly the fall) of one person, in the manner of Theodore Dreiser's *The Financier* (1940), or a group of people, as in Mary McCarthy's *The Group* (1963)—but whenever I contemplated these possibilities, I felt unsatisfied. Such a book could be dismissed as merely anecdotal; it would not have as its subject an important historical development, a thing that happened to many people at the same time.

What started the process of my getting unstuck was working on *The Promised Land*—specifically, my series of interviews with Constance Daniels, who lives in the world's largest and most notorious housing project, the Robert Taylor Homes in Chicago. Her apartment was only forty blocks south of the Loop; from the window were visible various social-service centers and public schools. Often, I would leave the apartment wondering, "How can someone so smart be so much outside the system, especially when the system is physically so nearby?" This question led to another question: "What *is* the system, exactly?"—something that, if you have always lived comfortably inside the system, it does not occur to you to ask. Once I had framed the issue in terms of a system, it occurred to me that if I could identify it and describe its construction, I would be able to provide my book on success with a narrative structure. The next step was to hypothesize that the system I was thinking of was the "meritocracy," the post-World War II institution of mass testing and mass higher education that sought to route the population to various destinations in the social structure. The development of the meritocracy could work as the backdrop against which I would be telling individual characters' life stories.

My next step was to write the Educational Testing Service in Princeton, New Jersey, and request access to its historical archives. This, I confess, was what we call in journalism a fishing expedition—I did not know whether there were any archives at the Educational Testing Service. But it turned out that there were and that they were voluminous. A historian named Gary Saretzky had been at ETS for almost twenty-five years meticulously building a library of primary-source material that almost no one outside of professional testing knew about. Also unbeknownst to me, just before my letter arrived, the then-octogenarian founding president of ETS, Henry Chauncey, had been contemplating inviting a non-academic historian (David McCullough and John McPhee were the names at the top of his list) into the archives to produce a history of educational testing. So I was lucky.

The problem that I have had at the ETS archives, and that even an academic historian might have, is being overwhelmed with primary-source material backed up by almost no secondary-source material. (I devoted a good deal of time to learning about and constructing a computerized "text database" of the archival material, as a way of trying to keep it all straight.) With the War on Poverty, I could swoop into an archive, spend a week picking out the memoranda of most interest to me, and fill in the details by consulting the work of Allen Matusow or another historian. I spent about a year at ETS, and even then I saw only a small portion of the material; on such basic points as when the Scholastic Aptitude Test was first widely administered, there is no published source, and the answer has to be found in letters and memoranda and by interviewing people. To be the first recorder of this one small part of the historical record gives me some feeling for the almost

frightening responsibility of historians. A journalist is usually covering familiar material, and the weight of knowing that your version of events is going to be the only extant version for a while is an unaccustomed one.

In sheer volume, there is enough ETS material for two or three books. Here, though, the institutional pressure at work on a non-academic historian has been healthy for me; I know that if my book appears to be about psychometrics rather than about success in America, it will not attract the size audience that I aspire to. Therefore, I have been pushing myself to broaden the story. First, in an effort to get outside of testing, I decided to include in the book the story of the meritocratization of Yale College in the 1960s. I picked Yale for typically narrative-related reasons. Of all the traditional elite schools, Yale traveled the farthest down the meritocratic road in the briefest time. Thus there were conflicts, which make good material for me. Also, because my topic is so broad, it is important to find ways to connect the interlocking stories I pick to illuminate the large theme. In the case of Yale, there is a wonderful natural link. The founding president of ETS was a man named Henry Chauncey. Henry Chauncey, Jr., was for many years a dean at Yale and, during the period I am interested in, was essentially the chief aide to the president of the university, Kingman Brewster. The jump-shift from ETS to Yale will not seem abrupt.

A couple of years ago, I was invited to give a lecture about my work-in-progress at the University of Wisconsin. While I was speaking, I got the disturbing feeling that my topic was registering with the audience as having to do with only a tiny subculture on the East Coast. Who cares, I kept hearing after the speech, who gets into Yale? Who cares about college admissions at all? (Wisconsin is a first-rank university that admits 71 percent of all applicants, including anyone in the top half of a high school class in the state.) The experience made me realize that I had to expand my narrative material, geographically and into the state universities, in order to drive home the idea that the meritocratic apparatus now constitutes the main “opportunity structure” (a term invented by Robert K. Merton, the sociologist) in the United States.

I began looking for a comprehensive blueprint for the remarkable expansion that had taken place in American higher education during the 1950s and 1960s. Although there are fewer of these than one might expect given the magnitude of the phenomenon, one did fit my bill almost perfectly: Clark Kerr’s book *The Uses of the University*.⁹ Kerr was not only a man with a vision, he was also head of the country’s biggest state university system and an effective politician: he induced the California legislature to pass a “master plan” for higher education that, in its original form, essentially guaranteed a nearly free, top-quality higher education as almost a right of citizenship. The history of the California master plan has been forgotten, and it is consequential: the way the master plan played out was crucial to the rise of Ronald Reagan and, possibly, will be crucial to the fall of Bill Clinton, who may lose the state of California in 1996 by virtue of being on the unpopular side of a citizen initiative to ban Affirmative Action in Kerr’s university system.

From my narrow perspective, it is very helpful that the first ETS branch office

⁹ Clark Kerr, *The Uses of the University*, 4th edn. (Cambridge, Mass., 1995).

was in Berkeley, that the University of California is ETS's biggest customer, and that Clark Kerr served on its board. Kerr is a wonderful interview subject, too. My hope is that, by interweaving ETS, Yale, and the University of California, I will have a broad, national institutional superstructure for my book. The next task, which I am engaged in now, is the search for a group of characters whose life stories will be blended with this institutional material.

IT STRIKES ME that there are two main shortcomings to the way in which I work, both of which arise from my overwhelming emphasis on casting historical writing in the narrative form.

First, narrative when done with skill feels true in some strangely automatic way. It is very close to being the basic means by which the human mind organizes experience. Therefore, it has enormous power to mislead. In narrative writing, the part always stands for the whole. The person you pick as an example of a vast phenomenon will appear in the work to represent the totality of the phenomenon. If the person is appealing, the phenomenon will look like a salutary development—and vice versa. There is almost always an implied message that inheres in the particularity of the story being told. Even if the non-academic historian constructs the narrative with scrupulous integrity, it still might not carry the message that it should.

It is this issue, in part, that has led the field of sociology to become overwhelmingly quantitative. History does not have that option. It is possible, anyway, to be aware of the perils of narrative and because of that be able to practice it more cleanly. Stories ought not to be seized upon simply because they are good stories; the meaning they convey should be one the author is aware of and wants to convey.

The second problem is the one that most concerned me when I began my book-writing career and that continues to bother me after all these years: the incompatibility of narrative and analysis. A great deal of the working process I have described is really a search for a narrative form in which to cast works of fundamentally analytic intent. The most natural subject for a non-academic historian is, as I have said, a biography, and lucky is the non-academic historian who can find a biographical subject who makes a perfect vehicle for the exploration of whatever issues are intellectually at hand. Sometimes, the reading public will respond to a non-narrative book if it has an extremely powerful premise (such books are easy to promote on television), but otherwise a story line has to be laboriously constructed that will seem natural to readers. Digression, exploration of a theme, speculation on broader meaning—these are luxuries for a non-academic historian, purchased through finding just the right tone of voice and way of framing the work, and even then they can be used only sparingly.

If academic historians sometimes have the problem of working in a hothouse atmosphere in which the issues of the profession are almost an obsession, non-academics like me have the opposite problem. Most of what I have been discussing here, I have never discussed with anyone. I am fortunate to have friends

in the academic profession who can be prevailed upon to read my work, but there is no regular mechanism for me to talk to my colleagues. Non-academics do not have a reliable way to acquaint themselves with the historical literature on their subject. They have no agreed-upon standards governing footnoting and attribution. There is not even a dialogue under way about how the problems associated with narrative writing might be more successfully addressed. In other words, it's lonely out here. I hope the future will bring some better mechanisms through which non-academic historians might bring some sense of overall progress to our unorganized field.

History, Language, and Reading: Waiting for Crillon

DOMINICK LACAPRA

A COMPARISON OF THE FIRST NUMBER of the first volume of the *American Historical Review* (October 1895) with recent issues of the journal might expectably reveal both continuities and discontinuities. Immediately evident is the basic format of substantive, monographic articles followed by book reviews. No longer retained is the special section on "documents." Still prominent, however, are the articles that contribute to research on specific topics. And the continued role of book reviews attests to their crucial function in establishing in practice the criteria of the discipline through quality control and policing procedures in which the selection of aspects of the work under review for special mention and the allocation of praise or blame affirm, reinforce, or attempt to revise disciplinary norms. An obvious yet important difference between "then" and "now" is indicated by the overwhelmingly Eurocentric nature of the early issues, in which the bulk of attention is lavished on the United States and England.

More striking, or at least less expected, a juxtaposition of the articles by William M. Sloane ("History and Democracy") and Henry Adams ("Count Edward de Crillon") might prepare one for recent controversies, notably debates bearing on the relation between history and deconstruction.¹ When one reads these inaugural articles together, it seems that Sloane takes the high road and almost appears as the "straight man" in sharp contrast to Adams as the doubting *ieron*. The very titles of their articles sound a significant difference, for in contrast to a "sit-up-and-take-notice" response to the weighty issues of "history and democracy," one is tempted to ask: Who was Edward de Crillon? And who cares?

For Sloane, history is a science, and his advocacy of objectivity may seem to imply the existence of an invariant characteristic of the discipline over time. But his very understanding of science introduces mutability, for it is decidedly shaped by his time. History for him is even more scientific than the natural sciences. He goes so far as to assert that, "paradoxical as it appears, the sciences of man's nature have for a generation past been growing more and more physical, just in proportion as the other sciences have been growing metaphysical." In addition, the consequences of "the doctrine of the unity of history" are "simply revolutionary, scientific methods having by its means been introduced into a discipline hitherto venerated as the highest department of prose literature, to be sure, but esteemed by the great critics, and by mankind generally, as on the whole vague

¹ William M. Sloane, "History and Democracy," *American Historical Review*, 1 (1895-96): 1-23; Henry Adams, "Count Edward de Crillon," *AHR*, 1 (1895-96): 51-69.

and imaginative, a picture of the writer's own mind rather than a presentation of facts in an external world, and of reliable deductions from them."²

Sloane's idea of history as unified science is founded on a theory of evolution and progress that makes it positivistic in a sense bearing certain resemblances to Auguste Comte's "positive philosophy" or Herbert Spencer's less grandiose emplotment of history as an evolutionary story. The notion of unity resonates both with an affirmed cosmopolitanism and with a conception of historiography centered on politics and the state, and it harmoniously joins science and democracy in a manner that permits "an instinctive conviction that with the advance of education and the spread of knowledge there has been a more or less perfect grasp of truth by an ever-increasing number of human beings, until now the majority is likely, in the long run, to decide upon any public question more correctly than the minority."³ Hence science does not exclude democratic faith—or even sustained Periclean musings about the glories and foibles of democracy in America that would seem more suitable for the inaugural address of a president of the United States than for the inaugural issue of a professional journal.

The linkage of science and democracy also prompts "the final question"—"whether [history] will continue to be literary in the old or in any sense." Here, Sloane once again invokes the principle of unity, for science may readily be reconciled with art in the interest of truth, notably because "the highest form of literary, as it is of historical, criticism is to separate the permanent from the transitory in its own age." An unself-consciously imperialistic and colonizing turn of phrase nonetheless indicates the way that Sloane's linkage of science and democracy and his uplifting belief in the evolutionary unity of mankind may not only downplay or obscure the role of difference and conflict in history but also involve a discursive exploitation of "savages": "At the same time we have laid the contemporary savage under contribution, and from him we have wrenched details for comparison with early institutions in regard to custom, myth, and society which seem likely to be of the first importance. The notion of chasms has disappeared, and the continuity of history has been established."⁴

Still, Sloane explicitly bemoans parochialism, takes a self-consciously cosmopolitan perspective, and, in his own voice as a member of the Board of Editors of the *American Historical Review*, stresses the role of constructive criticism. He even asks whether we may hope that in time the intelligent reader "will be on the lookout not for new information solely nor for erudite reference to archives, rare books and manuscript authorities alone, but for the method and spirit which constitute the intellectual personality of the writer, in order to judge not only of his industry but of his spiritual dimensions." In addition, he asserts the noble principle that the *AHR* "must by the auspices under which it begins, display the largest catholicity possible, and an impartiality willing always to hear the other side." For him, the *Review* should no doubt publish monographic research, but it "ought chiefly to be a critical review" of books—"fearless to denounce a bad or superficial

² Sloane, "History and Democracy," 2.

³ Sloane, "History and Democracy," 8–9, 6.

⁴ Sloane, "History and Democracy," 16, 17, 3–4.

book which solicits public favor, equally courageous to sustain one which presents unpopular truth, and sufficiently learned to give reasons for its opinions.”⁵ Thus the *AHR* should itself follow the democratic principles Sloane has taken pains to elicit and elaborate, although he does not specify the concrete means of implementing his high principles in the operative editorial policy of the *Review*.

By marked contrast with Sloane’s harmonizing and idealizing dedication to unity, Henry Adams introduces chasms into the very core of historiography itself, and he emphasizes its erring nature with an insistence that might bring to the mind of today’s reader the theories of Paul de Man. Adams’s topic seems less than transitory, and his vision of disconcerting and uncannily proliferating error in the work of the historian eventuates in an idea of the historian’s craft as threatened by melt-down into “an inextricable mess.” For him, “the historian is properly responsible only for his own personal error, but this he can never calculate, since it is hopelessly confused with the conditions of his education, his society, and his age . . . Some historians are more, some less, inaccurate; but the best must always stand in terror of the blunders which no precaution and no anxiety can save him from committing.” Indeed, such is the historian’s plight that, “conscious of the pitfalls that surround him, the writer of history can only wait in silent hope that no one will read him,—at least with too much attention.”⁶

After these hyperbolic words of caution, which are difficult to distinguish from ironic counsels of despair, Adams goes on to interweave “documents” with his own commentary in a rather bizarre and seemingly pointless effort to determine just who Count Edward de Crillon—mistakenly identified by James Madison as one of Napoleon’s secret agents—really was. This misidentification helped lead Madison to believe that Crillon would induce the Irishman John Henry to deliver, for the exorbitant sum of \$50,000 (Henry wanted even more money), secret papers that—Madison was willingly duped into believing—would implicate Federalist adversaries in compromising, even treasonous, dealings with England.

Adams asserts that the “blunder” involved in the identification of Crillon as an official French secret agent, which can be “found in Volume II, page 186, of the *History of the First Administration of Madison*,” is “fortunately of so little consequence as to allow of attaching a story to it.”⁷ If it is not taken as a warning signal concerning the role of “literary” techniques in Adams’s own account that are less easily harmonized with “science” than Sloane believed, this statement is suspiciously paradoxical, for it acknowledges the relative unimportance of Crillon’s identity yet asserts that this very status allows “attaching a story to it.” In history, one would expect a “story” to be warranted in relation to real matters of some importance.

Adams’s style is strongly reminiscent of that in *The Education of Henry Adams* (1907). Its ironic nature, along with the fantastic tale it unfolds, almost leads one to expect that Adams will follow novelistic convention by informing the reader that he found the documents relating to Crillon in a trunk or a bottle washed up on shore. In fact, he informs us that “a volume of the archives of the French

⁵ Sloane, “History and Democracy,” 19, 21, 22–23.

⁶ Adams, “Count Edward de Crillon,” 52, 51.

⁷ Adams, “Count Edward de Crillon,” 52.

Foreign Office, overlooked in the original search for documents relating to the United States, contains some papers relating to this matter, which seems at the time to have perplexed the French government almost as much as it annoyed Mr. Madison." The suspect "identity" that the hitherto-undiscovered archival stash reveals for Crillon is that of a chameleon-like confidence man who was originally named Soubiron and acquired the name of Crillon (as well as his benefactor's fortune) from a M. de Crillon Partorias, one of the many who confided in him.⁸ Adams himself tells the reader that "Soubiron was a lineal descendant from that society which the Spaniards called *picaresque*, and which had a literature of its own . . . Soubiron was a Gascon, and must have been a more or less plausible rogue." Adams even concludes that the man was "merely a common swindler."⁹ Indeed, aspects of Soubiron-Crillon's story are so implausible that they would not pass muster in serious fiction. While Adams was not a "common swindler," he had more than a touch of the picaro; and the sustained ironic style, hyperbolic skepticism, disorienting stress on blunders, ludic melancholy, and intertextual resonances (both with reference to *The Education* and to picaresque novels of adventure) may leave the reader somewhat perplexed, if not annoyed, about what precisely to make of his contribution to the inaugural volume of the *AHR*. In any case, its features may lead one to question a reliance on a golden-age mythology that nostalgically invokes the glories of a more "objective," less "deconstructive" past and bemoans a putative recent descent of the profession into epistemological and methodological disarray.

MY INTRODUCTORY COMMENTS in certain ways confirm the narrative (but not the apparently gloomy import of the "no-king-in-Israel" conclusion) of Peter Novick's *That Noble Dream: The "Objectivity Question" and the American Historical Profession*.¹⁰ They indicate that it is deceptive to rely on any simple "from/to" scenario in relating the past to the present of the discipline. The model Novick's treatment suggests is one of repetition with more or less significant—at times, seemingly traumatic—change. Disciplinary consensus on the ideal of objectivity has not only varied with changing or tensely interacting conceptions of science. It has been disturbed in the past by skepticism and doubt often seen as implying relativism, not only in Henry Adams but most notably at the time of Charles Beard, Carl Becker, and the New History. In certain ways, recent controversies about various "new historicisms" recall these earlier debates. But Novick himself sees the recent period not simply as bringing a heightened intensity of questioning but as involving a more specific focus on the problems of language and signification. This emphasis on language also typifies the influential review essay by John Toews

⁸ Adams, "Count Edward de Crillon," 53. For more on Edward (or Edouard) de Crillon, see Samuel Eliot Morison, *By Land and by Sea* (New York, 1953), 269–73; and Clifford L. Egan, *Neither Peace nor War: Franco-American Relations, 1803–1812* (Baton Rouge, La., 1983), 171–72.

⁹ Adams, "Count Edward de Crillon," 60. It is also curious that Adams ends his article abruptly with a letter from Soubiron to Henry (John Henry, not Henry Adams) on which Adams provides no further commentary.

¹⁰ Cambridge, 1988.

on the “linguistic turn,” which I shall discuss later in this article.¹¹ This turn, which of course has many and at times incompatible variants, is most fruitfully understood as involving a recognition of the problematic nature of language or any signifying practice (ritual or dance, for example).

Language in this sense is not a purely transparent medium that may simply be looked through (or bracketed) in the interest of (re)presenting the object or findings of research. It poses problems for the historian (or other analyst) and signals the manner in which the observer is constitutively implicated in the object of research. With reference to psychoanalysis, Freud framed this problem in terms of transference, and transference involves both the tendency of the analyst-analysand relation to repeat typically inappropriate parent-child relations and the more general tendency of an analytic discourse to repeat the problems at issue in its object of analysis (a tendency evidenced, for example, in Sloane’s repetition of imperialistic and colonial assumptions in his statement about the historiographical use of “savages”).¹² The goal for Freud was to pass from a perhaps inevitable and necessary tendency to “act out” (or compulsively relive) these problems—a tendency particularly insistent with respect to traumatic events—to the attempt to recall them in memory and critically work through them.¹³

The linguistic turn brings with it an openness to literary and critical theory, including aspects of (Continental) philosophy, in the effort to rethink the nature and acceptable boundaries of historiography. It also mitigates the stark dichotomy between history and metahistory, if by the latter one means critical and self-critical theory bearing on the practice of history itself. Insofar as the professionalization of the discipline was experienced as requiring boundary setting, and literature, literary theory, and philosophy were either positioned as largely negative identities or effortlessly harmonized with science in a deceptively ideal unity, then the recent turn to theory and the controversies it stimulates may be interpreted as a return of the repressed. Moreover, the varying fortunes of narrative in historiography indicate the role of a variable proximity to a certain dimension of literature and literary theory, but this proximity—both in those who cultivate and in those who excoriate it—has until recently been typically enacted or acted out rather than lucidly theorized. And the recent emphasis on narrativity has not brought consensus among historians either about the role of narrative in historiography or about the precise nature and status of narrative procedures in history and literature.¹⁴ The possibility that the turn to narrative theory (and the “literary”

¹¹ John E. Toews, “Intellectual History after the Linguistic Turn: The Autonomy of Meaning and the Irreducibility of Experience,” *AHR*, 92 (October 1987): 879–907. See also John H. Zammito, “Are We Being Theoretical Yet? The New Historicism, the New Philosophy of History, and ‘Practicing’ Historians,” *Journal of Modern History*, 65 (1993): 783–814. Zammito’s basic argument draws much from Toews’ article.

¹² See Dominick LaCapra, “History and Psychoanalysis,” in *Soundings in Critical Theory* (Ithaca, N.Y., 1989), 30–66; as well as *Representing the Holocaust: History, Theory, Trauma* (Ithaca, 1994).

¹³ See especially Sigmund Freud, “Remembering, Repeating and Working Through” (1914), in *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, vol. 12, James Strachey, trans. (London, 1958), 145–56; and “Mourning and Melancholia” (1917), in *The Standard Edition*, vol. 14 (1957), 237–60.

¹⁴ See, for example, Philippe Carrard, *Poetics of the New History: French Historical Discourse from Braudel to Chartier* (Baltimore, Md., 1992); Lionel Gossman, *Between History and Literature* (Cambridge, Mass., 1990); Hans Kellner, *Language and Historical Representation: Getting the Story Crooked* (Madison, Wis.,

more generally) may be a sign of a returning repressed also helps to explain the excesses in the gesture, including the tendency at times to cannibalize literary theory and to make an unmediated and uncritical application of it to historiography. Moreover, it induces opponents of the turn to run together those who have shown an interest in it and to castigate them in an indiscriminate manner as herdlike creatures in a night in which all cows are gray.¹⁵

There is a sense in which placing language or, more generally, signification in the foreground of attention and having it apply self-reflexively to the practice of the historian creates a crisis or at least a minor trauma in historiography. For this turn means that one cannot stay fixated on the object of research, construe this object in a purely objectified manner, and provide unproblematic, "sun-clear" reports about its nature. Moreover, one cannot situate language or signification in a merely instrumental and subordinate position, and it cannot be confined to the status of simply one more object of investigation. Indeed, with the turn to language, an entire research paradigm may be placed in question.

A relatively self-sufficient research paradigm was in certain ways important for the professionalization of history as a discipline, and attacks on tendencies that question it may be taken as one indication of the extent to which it is still understood (perhaps misleadingly) as essential to the discipline even today. This paradigm enjoins gathering and analyzing (preferably archival) information about an object of study in contrast to reading and interpreting texts or textualized phenomena. (In this exclusionary sense, reading a text, especially a published text, is *not* doing research.) In its self-sufficient form, which may be common to conceptions of history as science and as narrative art, the research paradigm is at least loosely modeled on a certain objectifying idea of science (or narrative) in which there is a definitive separation and relation of cognitive mastery between the observer and the observed. The observer puts forth certain theses or hypotheses about the observed that are subject to confirmation or disconfirmation through empirical investigation.

Obviously, important elements of this paradigm, such as the gathering and analysis of information or the testing of propositions, may be defended and distinguished from their role in what I am terming a relatively self-sufficient research paradigm, and the very concept of what counts as research may change (initiatives I deem desirable). Moreover, this paradigm or model should be seen as objectivist or one-sidedly objectifying rather than as simply objective, for it is possible to have a conception of objectivity that does not depend on it, and criticisms of it should not be seen as entailing an indiscriminate skepticism or a theory of history (or historiography) as endless erring in abyssal chasms.

An alternative conception of objectivity would stress the importance of thor-

1989); Louis O. Mink, *Historical Understanding*, Brian Fay, Eugene O. Golob, and Richard T. Vann, eds. (Ithaca, N.Y., 1987); and Hayden White, *Metahistory: The Historical Imagination in Nineteenth-Century Europe* (Baltimore, 1973); *Tropics of Discourse: Essays in Cultural Criticism* (Baltimore, 1978); *The Content of the Form: Narrative Discourse and Historical Representation* (Baltimore, 1987).

¹⁵ Indiscriminate polemic based on excessively homogenizing reading is pronounced in Bryan D. Palmer, *Descent into Discourse: The Reification of Language and the Writing of Social History* (Philadelphia, Pa., 1990); and Gertrude Himmelfarb, "Post-modernist History and the Flight from Fact," *Times Literary Supplement* (October 16, 1992): 12–15.

ough research and accuracy, while nonetheless recognizing that language helps to constitute its object, historical statements depend on inferences from textualized traces, and the position of the historian cannot be taken for granted. There would be an active awareness that such issues as the subject-position(s) and voice(s) of the historian are an integral component of historiography complicating research and that the elucidation of one's implication in a contemporary network of research and methodological-theoretical-ideological controversy is not simply a dispensable matter of "metahistory" or a specialized activity to be relegated to the "think-piece." Moreover, one could not rely on the idea that objectivity is a normal given of historiography assured by established procedures or that bias is a deviation from normality for which one may simply "correct." Rather, one's perspective would be transformed insofar as one recognized the constitutive place of the historian in the research project and saw objectivity not as the simple opposite of subjectivity but as a tenuous yet valuable goal of a process of elaborating a range of subject-positions (for example, those of researcher, reader, and theorist or intellectual) by negotiating "transferential" relations in a critical and self-critical manner. Research would be combined with reading and interpretation in a larger, more problematic conception of historiography in which the work of different historians would justifiably show different weightings and articulations and the decisive opposition between texts and documents would be questioned. Documents would be read textually, and the manner in which they construct their object in an institutional and ideological field would be a matter of critical scrutiny, while the documentary dimensions of texts would be posed as an explicit problem and elucidated.¹⁶

Reading in both its literal and metaphoric senses is a crucial constituent of the problem of language. And it is reciprocally related to writing. A mode of reading implies a mode of writing (and vice-versa). In this sense, a different mode of reading would imply that the writing of history would also undergo significant variations and that historical works might take different forms. I do not think there is any simple choice between research and practices of reading and writing, although there is a tense and at times agonistic relation between them. But the problem in historiography is to conjoin them and to attempt to determine what range of practices combines in an acceptable manner a revised understanding of research and modes of reading and writing (or, more generally, practices of

¹⁶ For a recent attempt to address these problems, see my *Representing the Holocaust*, chap. 1. The notion of subject-position points to the intersection of subject and society—the manner in which the subject is positioned and positions him or herself in society. It also indicates the need to relate psychological or psychoanalytic concerns to social, historical, and political issues. Any individual occupies a number of more or less compatible subject-positions that provide the starting point for a response to problems. "Identity" is a problematic articulation of subject-positions, and it cannot be reduced to either subjectivity or social roles and group affiliations alone. Moreover, one's subject-positions and "identity" can to some extent be transformed through social practice, including the manner in which one responds to problems. In any event, the purpose of critical thought is not simply to legitimate subject-positions but to acknowledge their role and at least enable their testing and possible transformation. One significant question is whether the professionalization of history has tended to confine the historian to one dominant subject-position—that of a relatively restricted conception of professional historian, notably in the context of a research paradigm—in a manner that obscures or excludes the role of other subject-positions, for example, critical reader and public intellectual.

signification). I am here assuming that historiography as a professional discipline need not be—and in fact has not been—predicated on one monological disciplinary practice but that it requires a certain coherence that can be satisfied by a range of practices evincing different emphases (for example, between research and reading or interpretation). What this range of practices is, however, should be seen as contestable and not subject to decision in an *a priori* manner. It should be a matter of informed argument and debate. In this sense, one should not be able to rule someone out of the profession in an apodictic manner because one disagrees with his or her practice.

Moreover, one should be open to the possibility that, in the event a certain practice is not “properly” historical, a given individual may combine it with historical practices in hybridized roles or subject-positions. The question of what modes of hybridization are acceptable would raise debates about historiography to another level and would reinforce the argument that the definition of historiography is contestable, perhaps essentially contestable. Such hybridized or cross-disciplinary positions could be seen as blurred only from within secure, decisive disciplinary enclosures, and such a view of them might well obscure newer articulations that are being formulated in and through them—articulations that may be most suitable for addressing problems that themselves cut across disciplines (such as the relation between text and context or between the present and the past). In practice, of course, decisions have to—and would be—made, and such material matters as whether new departments or programs should be instituted or who should be hired for a given position would provide a pragmatic court of appeal—but one whose determinations might well change over time with changing conceptions of the acceptable range of disciplinary practices. For example, one might decide that the hybridization of historian with critical theorist or public intellectual was acceptable, indeed, desirable if the latter role did not involve direct propagandizing in the classroom or the use of professional arenas, such as conventions or the pages of the *AHR*, for narrow partisan-political activities.

I WOULD PROPOSE that there are at present at least five important approaches to reading that are relevant to the practice of history. In proposing these approaches, it should be evident that I am presenting a typology of important reading practices, no one of which may inclusively and exhaustively define the activity of any given historian or group of historians (such as social or intellectual historians). Any given historian may employ or even combine two or more approaches, although it is often the case that one approach is most prominent in a historian's work; it shapes the character of inquiry and is used to determine what aspects of the other four are particularly useful or open to appropriation. In any event, there are more or less pronounced tensions involved in the combination of at least certain types with one another in discursive practice. (Thus, as we will see, William Sloane's “History and Democracy” would combine the first, second, and fourth categories, while Henry Adams's “Count Edward de Crillon” in its most unsettling

dimensions would approximate the third.) The goal of the typology is to locate important protocols of reading that cut across both thematic emphases on issues such as race, gender, class, and sexual orientation (prominent in Marxism, feminism, ethnic studies, and gay and lesbian studies) and disciplines (notably history, philosophy, social theory, and literary criticism), although certain protocols may be much more prominent in certain emphases or disciplines than in others. Problems of race, gender, class, and sexual orientation are of crucial importance, and they should be addressed. The question for further inquiry is whether they are best addressed through one or more of the protocols of reading I shall discuss—or whether another approach I do not envision would be better still.

1. The Denial or Repression of Reading. Here, the dominance of a seemingly self-sufficient research paradigm leads to an inability to recognize reading as a problem. All texts and documents are assimilated to a homogeneous status as sources or evidence that enable the determination of certain findings. Research findings are often “written up” rather than written in a stronger sense, and an unadorned plain style is favored. Typically, literary or philosophical texts are reduced to the status of unreliable sources because they do not yield solid evidence or clear-cut facts about empirical states of affairs. They tend to be excluded from the record or at most referred to in brief, allusive ways as possibly suggestive for research. In any event, whatever they yield must be checked against more reliable documents, thus rendering their status redundant.¹⁷

In this approach, a priority has often been placed on archival sources and extensive archival research in which the critique of sources is limited to validating the authenticity of documents.¹⁸ But this priority is in no sense necessary, and texts of both high and popular culture may be treated in accordance with a research paradigm for which reading is not a problem. Motives and ideological “bias” in those one investigates may of course be suspected, but such bias refers primarily to conscious intentions or well-defined strategies that may be established with the same certainty as the meaning of a text through straightforward reading of its content. Ideally, other texts (such as letters) give grounds for the ascription of ideological intentions that may be compared with what the manifest content of documents reveals. The goal is to elaborate either a particularistic or a more

¹⁷ In Louis Chevalier, *Classes laborieuses et classes dangereuses* (Paris, 1958), documents such as police reports are used to check the “findings” derived from novels, as if both police reports and novels did not require more complex protocols of reading. Of Eugène Sue’s *Mystères de Paris*, Chevalier writes: “The social importance of this novel, like that of other great novels of the time, comes from the fact that their authors describe a society and an epoch to which they belong . . . The extraordinary authenticity of *Les mystères de Paris*, like that of *Les misérables*, comes from the fact that these works passively register the demographic and economic evolutions that we have evoked. They are of their time and can do nothing other than attribute to the society they describe the characteristics that their authors know in the same manner in which they were known at the same moment by the most uncultivated [*incultes*] inhabitants of the city” (514–15; my translation).

¹⁸ I would note that the problem of reading in the archives has increasingly become a concern of those doing archival research, thus leading historians who do extensive archival work to become interested in problems raised later in this article. See, for example, Robert Darnton, *The Great Cat Massacre and Other Episodes in French Cultural History* (New York, 1984); Natalie Zemon Davis, *Fiction in the Archives: Pardon Tales and Their Tellers in Sixteenth-Century France* (Stanford, Calif., 1987); Steven L. Kaplan, *Adieu 89* (Paris, 1993); and Emmanuel Le Roy-Ladurie, *La sorcière de Jasmin* (Paris, 1983).

panoptic, panoramic account of a context in relation to which texts are strictly subordinate if not merely symptomatic documents. Instruction at the graduate level tends to take the form of devising research projects that emulate valued exemplars of successful research, thus avoiding the lures of approaches to history that do not conform to a self-sufficient research paradigm. Research itself is successful if it revises a hypothesis or retells a story in a manner that adds to, revises, or, in rare cases, overturns earlier respected examples of successful research on the basis of extensive (ideally, exhaustive), solidly grounded empirical inquiry.

At present, the above description may seem like a caricature, but the question is the extent to which the caricature still captures crucial aspects of actual practice and graduate training. To refer briefly to a “then” of which I still have a vivid memory, my own graduate training in the 1960s by and large conformed to this model. It was rare, even in a seminar in intellectual history, to discuss in common a text that all members of the class had read. Research seminars met for a few initial sessions in which possible research topics were brought up and perhaps a few models of research read but not discussed in any critical form that addressed their assumptions or manner of conceiving history. Then members of the class went off for six weeks or so to conduct independent research, following which they reassembled to report their findings and to benefit from the reactions of others in the class. Shortly after my period of graduate study, a methodology seminar was introduced in which there was discussion of common readings. But priority in selecting them was frequently given to very recent historiography, which might be directly emulated or trashed by aspiring professionals. In preparation for general examinations, the prevalent ideal was to read a book a day in accordance with a kind of academic Fordism that helped one accumulate an impressive array of bibliographical references and a synoptic knowledge of the contents or theses of various studies.

Whenever I am inclined to believe that at present the preceding caricature no longer applies, I encounter a historian who arises to enact or act it out even while he or she may want to dismiss it as inapplicable. To the extent my experience is representative, it bears witness to inner division and anxiety in the profession provoked by recent critical-theoretical initiatives. Still, certain procedures have been modified and in certain places (such as my own university) drastically overhauled, at least in some areas of history. These procedures embodied virtues that many (myself included) would like to retain in a different conception of research, reading, and graduate education—virtues such as the insistence on extensive (ideally, exhaustive) research concerning a topic; a thorough knowledge of literature relevant to one’s object of inquiry; an ability to conduct independent research; a concern for meticulous care in making statements or assertions and in validating their more empirical or constative aspects; and the judgment necessary to make significant distinctions and to frame explicitly (but not simply to exclude) as speculative or hyperbolic certain dimensions of an account or argument. But the limitations of a narrowly construed, exclusionary research paradigm need not be belabored, notably confinement of historical understanding to a restricted, constative, empirical-analytic model and unconcern

(if not disdain) for critical and self-critical theory.¹⁹ It is no doubt true that a lack of concern for theoretical problems, including the complexities of reading texts and documents, facilitates both a mellifluous, accessible writing style and the acquisition of large amounts of empirical information, while problematizing certain procedures can have an inhibiting effect or at least subject certain procedures to time-consuming and possibly doubt-creating critical processes. But the issue is not simply whether the gains of problematization outweigh the losses but whether certain procedures of exclusion are acceptable or even cognitively responsible once questions about them do arise and seem cogent.

2. Synoptic Reading. The synoptic approach to reading, typically with a focus on content or theme, in a sense makes explicit the practice of reading that is operative when reading is not taken as problematic in a research paradigm. It thus may help open certain practices to inspection and debate, enabling a more precise idea of their virtues and limitations. Furthermore, literary or philosophical texts may now be objects of extended study or even focal points of research. But the synoptic or paraphrastic approach remains geared to reporting the “findings” of reading or summarizing the meaning of large runs of texts or documents in a concise, lucid manner. Moreover, it downplays nuances and is geared to the reconstruction of the object, often to the exclusion (or occlusion) of a more dialogic, critical exchange with the past and its artifacts.

Synopsis may, of course, be the primary method of reading in intellectual and cultural history as well as in other subdisciplines. Typically, the goal of such reading is to derive reliable information, to state the manifest meaning of a text or document, and to develop some overarching thesis about a period, phenomenon, or development to which specific texts contribute primarily as symptom, illustration, or evidence.²⁰ One may at times grant priority to texts and documents

¹⁹ Even so important and sophisticated a historian as J. G. A. Pocock resorts to the invidious binary opposition between the “working” historian and his or her “other,” the metahistorian, thereby reducing an interest in critical theory to what might facetiously be called an “attitude problem.” Hence Pocock writes: “It is possible to define ‘intellectual history’ as the pursuit by the ‘intellectual’ of an attitude towards ‘history,’ and to write it as a series of dialogues between the historian himself, as intellectual, and his probably French or German predecessors, in the attempt to arrive at a ‘philosophy of history’ or something to take the place of one. Such ‘intellectual history’ will be metahistory, meaning that it will be reflection about ‘history’ itself. But it is also possible to imagine a ‘working historian’ who desires to be a historian but not (in this sense) an intellectual, who desires to practise the writing of history but not to arrive at an attitude towards it, and who does not look beyond the construction of those narrative histories of various kinds of intellectual activity which she or he knows how to write . . . It is such a working historian of this kind whom I have presupposed in this article.” *Intellectual History Newsletter* (1986): 8.

²⁰ A recent, extremely successful, and genuinely valuable instance of the synoptic approach is Steven E. Aschheim, *The Nietzsche Legacy in Germany, 1890–1990* (Berkeley, Calif., 1992). Aschheim writes: “We have had to sacrifice some of the complexity and creative intensity characteristic of so many of these individual encounters in order to retain a synoptic perspective . . . The philosopher is not only free to judge and evaluate—he is obliged to do so. Cultural historians, however, must be exceedingly wary of such exercises. It is the dynamic nature of Nietzsche’s influence, the complex diffusion and uses of his ideas, not their inherent truth, falsity, or even plausibility that must lie at the center of historical analysis . . . For the historian interested in the role, dynamics, and effects of ideas within a political culture, the question of valid or invalid interpretation and applications must be set aside” (pp. 3–5, 316). Yet, in contradictory or at least insufficiently examined fashion, Aschheim also wants to argue that something inherent in Nietzsche’s thought allowed or even invited the diverse and at times divergent interpretations and uses he so skillfully traces. “Nietzsche’s congeniality to so

from which facts can be extracted to reinforce or supplement one's reconstruction of a phenomenon or period. Texts (such as those of James Joyce, Virginia Woolf, Samuel Beckett, or even Jacques Derrida) that render precious little for this method may be declared to be unreadable, unintelligible, or obscurantist, and their authors deemed hermetic or nihilistic. The difficulty in some periods, including the modern one, is that so many texts and writers tend to fall into this category that the historian is inclined to develop reductive theses about their disastrous effects or their status as mere symptoms of the worst modernity has to offer. They by and large bear witness to a destruction of reason, a misguided departure from cherished Enlightenment ideals, a death-dance of principles, a past imperfect, or an "after-everything" phantasmagoria.²¹

The synoptic approach shares with the denial of reading a focus on the signified (or meaning) and the referents of texts to the virtual exclusion of a concern for the work and play of the signifier or, more generally, for the way a text does what it does. It is in good part for this reason that reading (and writing) remain relatively unproblematic in this approach. But one should recognize that synopsis and its attendant procedures remain a basic and important level of all reading concerned with meaning, reference, and the reconstruction of the object of study.²² Moreover, certain procedures that typically attend it are desirable, for

many contrary tendencies and interests and capacity to elicit open-ended responses reflected a central property of his post-Hegelian thought and method" (pp. 7–8). One danger in Aschheim's approach to reception is that it surreptitiously assumes an interpretation of Nietzsche as completely open-ended or even threatens to turn Nietzsche's texts into mere Rorschach tests and to eventuate in the uncritical belief that *esse est percipi*. Aschheim, however, goes on to allude briefly to such factors as Nietzsche's rejection of systems, his aphoristic style, his shifting narrative perspective or voice, and his sustained celebration of creativity (p. 8). Here, Aschheim in a minimal way does explicitly read, interpret, and enter into an exchange with Nietzsche's texts. But one might argue that, in light of the "centrality" he himself gives to them as well as to the contextual reconstruction of their reception, he might at least have raised certain questions about the factors to which he alludes. For example, does the rejection of system imply a disavowal of all systematicity or coherence? Are certain appropriations and uses of Nietzsche more defensible than others? Is an aphoristic style or a shifting narrative voice necessarily open to misinterpretation, or can it guard against certain abuses? One might well argue that Aschheim has done enough in the book he has written and that the exploration of such questions was not required of him. But one might nonetheless insist that inquiry into such questions, even when one is unable to answer them in a fully adequate or satisfactory manner, is permissible and even desirable for the historian and that such inquiry is demanded to complement and supplement the truly informative kind of contextual history of reception that Aschheim provides. Moreover, such an insistence would entail a closer and more interactive relation between the historian and the philosopher than Aschheim envisions.

²¹ For a range of perspectives on these problems, see Georg Lukács, *Die Zerstörung der Vernunft* (Berlin, 1954); Jürgen Habermas, *The Philosophical Discourse of Modernity*, Frederick G. Lawrence, trans. (Cambridge, Mass., 1987); Carl E. Schorske, *Fin-de-Siècle Vienna: Politics and Culture* (New York, 1980); Tony Judt, *Past Imperfect: French Intellectuals, 1944–1956* (Berkeley, Calif., 1992); John Lukács, *The Passing of the Modern Age* (New York, 1970); and Roland N. Stromberg, *After Everything: Western Intellectual History since 1945* (New York, 1975). A. J. P. Taylor writes: "Literature tells us little when we deal, as we must in the twentieth century, with the people of England. The novels of Virginia Woolf, for example, were greatly esteemed by a small intellectual group, and their destruction of the tight narrative frame has influenced later writers. They are irrelevant for the historian." Taylor, *English History, 1914–1945* (New York, 1965), 311. (I thank Jonathan Sadowsky for calling my attention to this reference.) "A small intellectual group" is apparently not part of "the people" for Taylor, and the irrelevance of Woolf's novels is so obvious that an apodictic non sequitur is sufficient to establish it.

²² For a cogent defense of synopsis that is also sensitive to certain of its limitations, see Martin Jay, "Two Cheers for Paraphrase: The Confessions of a Synoptic Intellectual Historian," in *Fin-de-Siècle*

example, the insistence on thorough research, the importance of substantiating empirical statements, and the careful distinction between empirical and more speculative assertions—procedures that are ingrained as common sense in professional historiography. This insistence may at times be misplaced insofar as it inhibits or invalidates more insistently interpretive or speculative ventures even when they are clearly framed as such. But it is nonetheless valuable as a characteristic of research and a check on more extravagant tendencies in reading and interpretation.

It is arguable that the synoptic approach and procedures related to it have become hegemonic or conventional in the historical discipline. But what is taken to be conventional changes over time and with different disciplines or discursive-institutional areas of society, and the attempt to determine what is or is not hegemonic in a complex field, especially at a time of intense controversy and change, is tentative at best. For example, New Criticism may now be conventional, even old-fashioned in literary criticism, and it did pay attention to the signifier, if only in a restricted, formalistic manner geared to the discovery of formal principles and the integrating role of irony and paradox. In historiography, by contrast, New Critical formalism is not conventional, and certain theorists who, from a literary-critical point of view, might be seen as in good measure New Critical (such as Hayden White in *Metahistory*) may be taken as radical or revolutionary within the historiographical field. Indeed, the goal of a counter-hegemonic practice (in contrast to an endlessly transgressive or anarchistic one) is to establish new conventions and norms in a discursive practice, although the norms deemed more desirable may be more open and self-questioning, notably with respect to the role of hybridization and the need for contestation or periodic transgression.

3. Deconstructive Reading. Deconstruction is of course a complex, heterogeneous movement that has been more prominent in literary criticism and certain branches of Continental philosophy than in history. But historians have shown interest in it, if only to learn enough about it in order to criticize it and its “lures,” often, if not typically, understanding it in rather reductive or truncated terms.²³ But, given the complexity of deconstruction and the variety of ways in which it is construed or employed, it is difficult not to be reductive in making generalizations

Socialism and Other Essays (New York, 1988), 52–63. For an erudite, magisterial synoptic history of the problem of vision in recent French thought, which includes elements of critical, dialogic exchange, see Jay, *Downcast Eyes: The Denigration of Vision in Twentieth-Century French Thought* (Berkeley, Calif., 1993).

²³ See, for example, James T. Kloppenberg, “Objectivity and Historicism: A Century of American Historical Writing,” *AHR*, 94 (October 1989): 1011–30. Contrast the more positive and constructive approach to deconstruction in Joan Wallach Scott, *Gender and the Politics of History* (New York, 1988), or Dominick LaCapra, *A Preface to Sartre* (Ithaca, N.Y., 1978); *Rethinking Intellectual History: Texts, Contexts, Language* (Ithaca, 1983); and *Soundings in Critical Theory* (Ithaca, 1989). Rather than repeat some of the things I have written about deconstruction, I shall try to take the discussion in somewhat different directions related to my understanding of some recent developments in deconstruction itself.

about it or its more prominent practitioners, and I cannot claim to have escaped this difficulty in my own discussion.

In deconstructive reading, there is a pronounced suspicion of synoptic or contextual reductionism, and virtually everything is to be found in nuance and the close reader's response to it. This approach to reading often brings extremely dismissive reactions to synoptic, content-oriented, and constative (or representational) reading practices—reactions especially evident in radical forms of deconstruction. Paul de Man and those modeling themselves on him tend to be radically deconstructive in the sense I am invoking. The writings of Derrida are more divided and at times involve countervailing forces, although they have a strong pull in a radically deconstructive direction that has perhaps been exacerbated with de Man's death and Derrida's inclination at times to identify with his theoretical views and reading practices.²⁴ In any case, the problem of the relationship between de Man and Derrida over time has received inadequate analysis because of the prevalent tendency of American deconstructionists to amalgamate the writings of de Man and Derrida into an insufficiently differentiated deconstructive reading practice or mode of criticism.²⁵

By radical deconstruction, I mean the tendency to take the important resistances to meaning and the internal contestations or tensions in texts and to become fixated on them by reading all texts in terms of an almost compulsively repeated process of locating an aporia, *mise en abîme*, uncanny nodal point, or process of internal undoing. The resistance to meaning thus threatens to become an externally predictable but internally compelling evacuation of meaning, and all roads in reading seem to lead to the aporia. Indeed, meaning (or the signified) tends to be eliminated or at least bracketed, and attention is riveted on the enigmatic play of the signifier that becomes arbitrary, mechanical, inhuman, "free" play. Moreover, the valuable emphasis on the conditions of possibility of a phenomenon or a historical process may be absolutized such that these conditions displace rather than inform history and lead to an abstract, meta-metaphysical mode of analysis in which specificity is lost or obscured.²⁶

In de Man, there is a marked contrast or unstable binary opposition between

²⁴ See Jacques Derrida, *Mémoires for Paul de Man*, Cecile Lindsay, Jonathan Culler, and Eduardo Cadava, trans. (New York, 1986); and "Like the Sound of the Sea Deep within a Shell: Paul de Man's War," Peggy Kamuf, trans., in *Critical Inquiry*, 14 (1988): 590–652 (reprinted in a slightly revised form in *Responses: On Paul de Man's Wartime Journalism*, Werner Hamacher, Neil Hertz, and Thomas Keenan, eds. (Lincoln, Neb., 1989). See also my critique of the latter essay in *Representing the Holocaust*, 125–33.

²⁵ For a lucid discussion of this problem, see Jeffrey T. Nealon, "The Discipline of Deconstruction," *Publications of the Modern Language Association*, 107 (1992): 1266–79.

²⁶ One may detect, in the following quotations from *The Resistance to Theory* (Wlad Godzich, trans., Minneapolis, Minn., 1986), de Man's tendency to construe literary theory as an autonomous, self-referential, involuted inquiry into pure conditions of possibility and their attendant resistances or aporias: "Literary theory can be said to come into being when the approach to literary texts is no longer based on non-linguistic, that is to say historical and aesthetic, considerations or, to put it somewhat less crudely, when the object of discussion is no longer the meaning or value but the modalities of production and of reception of meaning and of value prior to their establishment—the implication being that this establishment is problematic enough to require an autonomous discipline of critical investigation to consider its possibility and its status . . . If these difficulties are indeed an integral part of the problem then they will have to be, to some extent, a-historical in the temporal sense of the term . . . Such difficulties can be read in the text of literary theory of all times, at whatever historical moment one wishes to select . . . Nothing can overcome the resistance to theory since theory is itself this resistance" (pp. 7, 12, 19).

hermeneutics and poetics. Hermeneutics is interpretation that seeks the meaning of texts, while poetics focuses on the play of the material signifier that is both the condition of meaning and the force that prevents meaning from being satisfying or even at times from constituting itself in any intelligible manner.²⁷ De Man sees an inevitable “fall” into hermeneutics even in the most “rigorous” critical practice, but this movement to meaning is evidently a “fall” in the strong sense, bringing with it displaced religious connotations. The ideal is an ascetic practice of criticism in which rigor entails resistance to meaning—particularly unearned meaning—and its seemingly all-consuming ideological lures. (De Man himself repeats this oppositional structure leading to an aporia in various guises, for example, in terms of the relation between metaphor and metonymy or semiotics and rhetoric.²⁸) A crucial question in reading de Man is whether the aporia marks a terminal impasse or immobilizing disorientation—inducing the endless acting out of a repetition compulsion, including a compulsive mode of analysis—or whether (as Derrida generously believes) it also helps to generate new questions and possibilities, thus opening at least provisional paths to working through problems.²⁹

In Derrida, one may distinguish between two related processes that have different weights in Derrida’s various writings: deconstruction and dissemination. Deconstruction involves the analysis of the tensely related, internally “dialogized” forces in a text that place its author’s explicit goals or intentions in more or less extreme jeopardy. Most prominently, an author’s reliance on traditional binary oppositions (inside/outside, identity/difference, male/female) and an attendant desire for totalization may be upset, undercut, or disoriented by less readily classifiable, more disconcerting movements in a text that leave undecidable residues, remainders, or hybridized elements. Since binaries are typically arranged in a “violent” hierarchy, deconstruction involves interrelated movements or phases: reversal (wherein the subordinate term is given priority) and generalized displacement. The latter movement requires an attempt to counteract the simple inversion of hierarchy that remains within a given frame of reference and establishes a new form of dominance, but where it leads is less clear. It often involves generalizing the residue or remainder and refiguring the subordinate term until one has a less stable field of meaning that may suggest (while never quite seeming to work out) newer articulations. It may also lead in the direction of dissemination where wordplay is pronounced and articulations are at best elusive and extremely labile. In any case, deconstruction does not simply exclude or bracket meaning and reference; it places them within a general text or interplay of textualized traces that they do not simply transcend or master. The notorious statement that “there is no outside-the-text [*il n’y a pas de hors-texte*]”³⁰ certainly questions any idea of full, unmediated presence (for example, of an experience, meaning, or divinity), but it does not eliminate all meaning or reference. Rather, it situates meaning and reference within a network of instituted

²⁷ See de Man, *Resistance to Theory*, esp. 55–56.

²⁸ See Paul de Man, *Allegories of Reading: Figural Language in Rousseau, Nietzsche, Rilke, and Proust* (New Haven, Conn., 1979), esp. chaps. 1 and 3.

²⁹ For Derrida’s view of de Man on the aporia, see *Mémoires for Paul de Man*, 132.

³⁰ Jacques Derrida, *Of Grammatology*, Gayatri Chakravorty Spivak, trans. (Baltimore, Md., 1976), 158.

traces or a generalized trace-structure, and it makes their situation a matter of a function within, or an inference from, such a "structure."

For Derrida, moreover, a text never simply conforms to its author's intentions, and its language is never fully transparent. More specifically, a text puts into play longstanding assumptions of the metaphysical tradition—assumptions that are not themselves fully homogeneous and that receive more or less critical and explicit formulation in the texts of significant philosophers. In his early "Structure, Sign, and Play in the Discourse of the Human Sciences," Derrida provided this principle for distinguishing between the relative critical and self-critical strengths of texts: "But if nobody can escape this necessity [of being within traditions and, in the West, within the tradition of inherited metaphysical concepts], and if no one is therefore responsible for giving in to it, however little, this does not mean that all the ways of giving in to it are of equal pertinence. The quality and fecundity of a discourse are perhaps measured by the critical rigor with which this relationship to the history of metaphysics and to inherited concepts is thought."³¹ In his discussion of Paul de Man's World War II journalism ("Like the Sound of the Sea Deep within a Shell: Paul de Man's War"), Derrida himself seemed to forget this important principle, for there he tried unconvincingly to argue that a propagandistic, ideologically saturated text that remained rather blindly within anti-Semitic stereotypes could be read to reveal inner tensions if not strong forms of self-deconstruction or self-questioning implying self-reflexive rigor. The result was not only apologetic with reference to de Man. It threatened to reprocess the object of study in terms of a rashly generalized reading technology and unconsciously to validate the most hostile and extreme claims of critics that deconstruction could be used tendentiously to rewrite the past and prove virtually anything.

Another way of seeing Derrida's unfortunate article on de Man is to read it as a misapplication of disseminatory writing that in other areas may be justifiably experimental or thought-provoking. Disseminatory writing may be related to, but not conflated with, another practice in recent criticism that stems from the work of Harold Bloom: strong misreading. Bloom sees strong misreading as a typical way in which major poets relate to dominant, father-like predecessors, but it may characterize criticism that emulates poetry or "creative" writing. Indeed, Derrida's article is an exceptionally "strong" and "creative" misreading in that it takes what is often weakest in de Man's early writing and makes something powerful and even subversive out of it. This procedure may be praiseworthy as an act of "creative" writing when it is a question of Beckett rewriting Gustave Flaubert, or Derrida rewriting Jean Genet, but it is altogether misplaced as historical reading and critique.

Dissemination in general supplements deconstruction through an active intervention in which a text is indeed rewritten in terms of possibilities that were underexploited or even unexplored by its author and perhaps remain submerged in the text. At its most extreme, this rewriting is a ludic improvisation that follows associative processes of a waking dream, making more or less regulated and lucid

³¹ Jacques Derrida, *The Structuralist Controversy: The Languages of Criticism and the Sciences of Man*, Richard Macksey and Eugenio Donato, eds. (Baltimore, Md., 1970), 252.

use of the processes Freud disclosed in dream-work (condensation, displacement, secondary revision, and suitability for staging or *mise en scène* [*Darstellbarkeit*]). Disseminatory writing thus enacts or acts out “free” association and dream-work, especially in response to a trauma that disrupts a text (in the broad sense that may include a life).

In Freud, of course, “free” association is not simply arbitrary; it brings out significant relations that may be repressed or denied by consciousness. The most effective associations are “free” in this disclosive sense. Reading that follows associative processes is thus a procedure that emulates psychoanalytic mechanisms. Its performative quality indicates that it does not simply copy or imitate the manifest content of the text being read but actually makes something happen (or makes history in its own way) through its associations and improvisations. Oneiric improvisation accounts for the “through-the-looking-glass” quality of certain deconstructive readings where what is made of a text departs drastically from anything a more conventional reading might reveal. Moreover, disseminatory writing and strong misreading may put into play a practice of radical decontextualization or diremption whereby textual segments are severed from their own time or place and made to take on new, unheard-of significations in their rewritten form.³²

In literary criticism that emulates creative writing, a reading may be praised to the extent that it is a strong misreading and engages its object in an unpredictable, even strangely disconcerting or uncanny, performative manner. Indeed, the stronger the misreading the better insofar as the strength of a misreading is indicative of the extent to which it appears performative, creative, or even original and brings out what is not evident in the text that becomes its pretext. The process is most remarkable when the resultant reading is as ingeniously creative as the text being read, for example, in many of Derrida’s texts (notably *Glas*) or in Roland Barthes’s *S/Z*, where Honoré de Balzac’s *Sarrasine* is decomposed and put together again in a fashion that would have astounded Humpty-Dumpty. Disseminatory writing as well as strong misreading is somewhat comparable to the “riff” in jazz wherein one musician improvises on a tune or on the style of an earlier musician. As in jazz, the more traumatic or disjunctive variations (or changes within repetition) may from a certain point of view be the most impressive. In this respect, Derrida may perhaps be seen as the John Coltrane of philosophy or of some hybridized genre it remains difficult to name.

It is, of course, difficult to determine whether a style emulating dream-work generates associations that are genuinely disclosive about both the text being read and a contemporary relation to it. Moreover, the improvisational procedure may border on the questionable, even in literary criticism or philosophy, when it becomes overwhelmingly projective reprocessing, in which the “voice” or perspective of the other is not attended to but is assimilated into a participatory discourse or generalized “free indirect style” that amounts to a monologic approach, however internally dialogized or self-contestatory it may be. This is, for example, the style in which Michel Foucault’s *Folie et déraison: Histoire de la folie à*

³² See Jacques Derrida, “Signature, Event, Context,” in *Margins of Philosophy*, Alan Bass, trans. (Chicago, 1982), 307–30.

*l'âge classique*³³ is often written, and it is one reason why the book is “exciting” as a mode of writing but questionable as history. In it, the “voices” of those classified as “mad” or radically “other” are not directly quoted or even commented on but are made to agitate Foucault’s own tortured, flamboyant prose and his internally divided, self-questioning style—a style that is internally dialogized but may still in an important sense be monologic (or narcissistic) insofar as it assimilates or incorporates the voices of others without respecting their resistances to assimilation. An internally dialogized, indeed, radically fragmented style that projectively reprocesses the voices of others may be lucidly theorized and defended as “schizophrenic” writing that attempts to break up the “paranoid” rigidities of classical or conventional writing. But the dangers are obvious in the view that a desirable alternative to paranoia is a strategic, willed release of “schizo” flows of energy and desire.³⁴

The preceding discussion may enable a delimitation of the point at which deconstruction and its disseminatory supplement—and, more generally, certain poststructural tendencies—have the most dubious relation to historical reading and interpretation. One may argue that historical reading should pay close attention to the “voices” of the other and try to reconstruct these “voices” or perspectives as closely and carefully as possible, however problematic the undertaking may be. This is one reason why contextualization is important even in approaches stressing the significance of reading and interpretation, although one may certainly debate the precise form and limits of contextualization in historiography.³⁵ One may also argue that the historian should elicit the possibilities and the repressed or denied dimensions of texts or documents. But the attempt to specify these dimensions of a text or phenomenon is always tentative, problematic, and even speculative. At the minimum, one would demand in historical reading and interpretation a specification of a configuration (or an articulation of the repeated and the changed) that would help to delimit better what may be ascribed to the past and what is being added in the present. Moreover, in history, the principle cannot be that the stronger the misreading the better, for here history does not emulate creative writing and is constrained by different norms of inquiry. At the very least, there is in history a basic distinction between the attempt to reconstruct the object of inquiry, including its meaning or possibilities at its own time or over time, and the entry into a dialogic exchange with it that tries to bring out its potential in the present and for the future. The question is whether deconstruction, disseminatory writing, and related poststructural tendencies tend to conflate or collapse the distinction between reconstruction and dialogic exchange through a kind of generalized free indirect style or middle voice that may neutralize or collapse not only binary oppositions but all distinctions.

³³ Paris, 1961.

³⁴ See, for example, Gilles Deleuze and Félix Guattari, *Anti-Oedipus: Capitalism and Schizophrenia*, Robert Hurley, Mark Seem, and Helen R. Lane, trans. (New York, 1977); and *A Thousand Plateaus*, Brian Massumi, trans. (Minneapolis, Minn., 1987). These books seem to take as their premise a naïve, utopian conception of desire opposed to all forms of limiting norms that are indiscriminately identified with repression and paranoia or even with fascism.

³⁵ On this problem, see my *Rethinking Intellectual History*, esp. chap. 1; *History and Criticism* (Ithaca, 1985); and *Representing the Holocaust*, esp. chap. 1.

It may, however, be the case that the historian may take up another role and engage in freer variations or speculations insofar as they are explicitly framed as such and not placed on the same level or indiscriminately intertwined with other readings and interpretations that are more explicitly controlled and subject to ordinary processes of validation. Here, one would have a hybridized role or genre of writing that is more or less close to Derrida's own. But how justifiable the specific instances of this genre might be is a subsequent question requiring further investigation and argument. As I noted, its use in the case of Derrida's rewriting of de Man's propagandistic journalism is objectionably indiscriminate and converges, even if unintentionally, with revisionism in the limited sense of a normalizing view of the Nazi period and the Shoah. Such a hybridized genre involving clearly framed speculations would seem most justifiable or acceptable to the extent that it is indeed difficult to determine what belongs to the past and what to the present, notably with respect to deep-seated philosophical assumptions and the repressed dimensions of a culture or tradition, for example, those related to a quest for origins, full presence, and purity that may be conjoined with displaced sacrificial and scapegoating mechanisms. But even here it should not lead to the loss or elimination of all historical specificity in treating phenomena.

I would make further mention of one important tendency in contemporary criticism that is evident in many writers sympathetic to deconstruction and at times in Derrida himself.³⁶ This is an ethos of renunciation combined with a propensity for a generalized aesthetics of the sublime.³⁷ Renunciation appears in methodological humility or modesty that avoids direct, "aggressive" criticism of the other as well as in the quest for an undecidable "position" that neutralizes or disempowers opposites such as activity and passivity. It also arises in the tendency to abandon not only mastery but seemingly all control in the more disseminatory extreme where language in its most hyperbolic forms takes over and, in a self-sacrificial or immolating gesture, distributes the decentered self in the flow of discourse. (This stance may be most appealing to men who are striving for sensitivity, especially in the face of feminist critiques of "phallocentrism" and patriarchy. In the case of women or minorities, it may have the tendency to reinforce unfortunate stereotypes of abjection.) The *mise en abîme* of the text may even coincide with the much-heralded (and greatly exaggerated) death of the author: language is presumably left to write itself in unpredictable, aleatory movements reminiscent of automatic writing in surrealism and open to the possibility (or necessity) of misreading. Yet the movement toward the abyss, in which all meaning and reference threaten to be lost, brings with it the possibility of reaching infinite heights—or at least infinite deferral of meaning—as the self

³⁶ See especially Leo Bersani, *The Culture of Redemption* (Cambridge, Mass., 1990). In opposing both conventionality and totalizing tyranny, Bersani elaborates a theory of presumably nonviolent masochistic desire in which the explosively narcissistic self seeks "the ecstatic suffering of a pure ébranlement" (p. 38) or self-shattering, thus averting frustration that, for Bersani, causes violence and aggression against others. Here, one may ask whether Bersani's intricate, probing, and demanding analyses, which provide arresting insight into the problematic nature of modern writing, seem at times to be based on a utopian anarchism close to the viewpoint of Deleuze and Guattari.

³⁷ See the independently developed but convergent analyses in my *Representing the Holocaust*; and Steven Connor, *Postmodernist Culture: An Introduction to Theories of the Contemporary* (Oxford, 1990), esp. chap. 8.

confronts symbolic excess (or dire lack) that simulates death and transfiguration. Trauma (including the induced trauma of the *mise en abîme* of the text) is transvalued in an aesthetics of the sublime into an occasion for ecstasy and exhilaration.³⁸ And the very ability to confront death, however symbolically, empowers the self and brings an intimation of the sublime as one steps away from the near annihilation threatened by the “abyss” of a radical excess or lack of meaning.

The fascination with excess that gestures toward death and a secular sacred—indeed, toward a secularized, symbolic sacrificial process that may paradoxically undo itself by playing itself out—is a powerful force in poststructural thought in general, and it harks back to the work of Georges Bataille, Maurice Blanchot, and Martin Heidegger. The problem is that its fascination is often so great that it induces a quasi-transcendental mode of thinking in which the deconstruction of metaphysics—including the very important deconstruction of the binary logic that subtends scapegoating and victimization—continues to gravitate in a meta-metaphysical orbit. To the extent that the inverted or parodic tracking of metaphysics prevails, the role of history and politics loses its specificity and becomes at best allusive, and any sense of limits or guardrails (whose importance Derrida himself at times invokes) easily becomes only a faint memory. The actual victims of history may be lost in an indiscriminate generalization of victimage (or abjection), a leveling identification of all history with trauma, and an abstract, tortuous conception of witnessing, all of which tend to have apologetic functions and to incapacitate all knowledge, judgment, and practice.³⁹ The attendant difficulty is a near fixation on trauma, as well as its transvaluation in the sublime, that induces a compulsively repetitive acting out of traumatic crisis and disorientation.

Deconstruction to some extent relies on a homeopathic process in creating discursive antidotes—antidotes that use a proper dosage of a prevalent cultural malaise (such as unlimited excess) in the attempt to facilitate a cure.⁴⁰ But when excess, or that which is beyond determination and representation, is fixated on, there is a marked inclination to overdose on the antidote. Moreover, that which can indeed be represented even in extreme experience or limit-cases is passed over in silence or treated in overly indirect, vague, and at times confusing terms. At this unrestrained, hyperbolic point, any more comprehensive working-through of problems tends to be foreclosed, and the sublime itself may become somewhat routinized and threadbare insofar as it is repeatedly alluded to, in increasingly

³⁸ For this transvaluation, the works of Jean-François Lyotard and Slavoj Žižek are especially significant. See especially Lyotard, *The Postmodern Condition: A Report on Knowledge*, Geoff Bennington and Brian Massumi, trans. (Minneapolis, Minn., 1984); *The Differend: Phrases in Dispute*, George Van Den Abbeele, trans. (Minneapolis, 1988); and Heidegger and “The Jews,” Andreas Michel and Mark S. Roberts, trans. (Minneapolis, 1990); Žižek, *The Sublime Object of Ideology* (London, 1990); *Looking Awry: An Introduction to Jacques Lacan through Popular Culture* (Cambridge, Mass., 1991); and *Enjoy Your Symptom! Jacques Lacan in Hollywood and Out* (New York, 1992).

³⁹ These tendencies are especially marked in the discussion of de Man in Shoshana Felman, “Paul de Man’s Silence,” *Critical Inquiry*, 15 (1989): 704–44 (reprinted as “After the Apocalypse: Paul de Man and the Fall to Silence,” in Shoshana Felman and Dori Laub, M.D., *Testimony: Crises of Witnessing in Literature, Psychoanalysis, and History* (New York, 1992), 120–64.

⁴⁰ One of Derrida’s best discussions of the antidote is his “Plato’s Pharmacy,” in *Dissemination*, Barbara Johnson, trans. (Chicago, 1981).

tedious, predictable terms, and has no strong resistances against which to assert itself. One may even have what Flaubert in *The Sentimental Education* (1869) termed “the sublime at bargain-basement prices [*le sublime à bon marché*].”

4. Redemptive Reading. The more extreme form of deconstruction or its disseminatory supplement may, as in de Man, lead one repeatedly to the abyss, with any redemptive gesture adamantly refused. Or it may hint, however obliquely or paradoxically, at redemption through a secularized sacrificial process and an aesthetics of the sublime. Another variant of reading, often in opposition or reaction to deconstruction, is more prevalent in historiography; it takes a rather sober if not placid approach to the redemption of meaning in and through interpretation. Close reading or attention to nuance is not the forte of this tendency. Indeed, reading becomes integrated into harmonizing interpretation, especially when a neo-Hegelian frame of reference explicitly encourages a model of speculative, dialectical transcendence that is often combined with a phenomenological notion of experience as the foundation of meaning. And interpretation is easily reconciled with the most conventional use of contextualization as the full meaning of a text or experience is presumed to be available to the interpreter through an attempt to capture the meaning-in-context of a past text or phenomenon—even of an entire series of events or sweep of a tradition. But redemptive reading often leads to a projective reprocessing of the past that is more secure and self-satisfying in its results than that operative in deconstruction, for the meaning redeemed is typically that which one desires in the present, and figures in the past tend to become vehicles or mouthpieces for contemporary values. This possibility is perhaps most available in its extreme form for historians and theorists who employ a neo-Hegelian frame of reference that applies to history the model of the speculative dialectic, in which a past phenomenon is “transcended” or “sublated” (*aufgehoben*) in time with only minor and essentially recuperable losses. Thus, where radical forms of deconstruction tend to evacuate meaning even where it seems to be significant, hermeneutic approaches may find ultimately satisfying or full meaning by filling in or covering over traumas and gaps that would seem to mark its limit.

The more moderate advocates of this approach to history as the story of “symbolic” meaning may look to Clifford Geertz and a delimited “anthropological” understanding of the nature of historical inquiry. Geertz’s work is especially appealing because of its remarkable insights and relative accessibility as well as its stylistic charms, which often are quite distant from the more difficult or even “rebarbative” nature of deconstructive and, more generally, poststructural approaches. Moreover, it provides a stimulating alternative to cut-and-dried approaches, especially those relying on “number-crunching.” It also allows for a measure of fictionalization not as pronounced or blatant as that evoked by strong misreadings—one that supports a turn to narrative in historical writing. The beautifully orchestrated, finely crafted, arrestingly dramatized narrative of the Balinese cockfight may here be seen as a *locus classicus* for a certain kind of history,

and Geertz himself often seems to be the *genius loci* in the work of a significant number of historians.⁴¹

More recently, the influence of Charles Taylor has brought into renewed prominence a neo-Hegelian model that is insistent and comprehensive in its construction of history as the story of meaning. This hermeneutic approach may be combined with a phenomenological notion of experience, as it was in earlier theorists and historians such as Wilhelm Dilthey. But I think that the redemptive model of reading is quite prevalent, often on a non-theoretical, conventional level in which there is a determined striving to seek out the meaning of past experience, frequently in terms that put into play or even help to validate contemporary desires and values.⁴² In a recent publication, I have discussed in terms of redemptive reading two important, extremely readable, yet exceptionally sophisticated books on a theoretical level: Caroline Walker Bynum's *Holy Feast and Holy Fast: The Religious Significance of Food to Medieval Women* and Charles Taylor's *Sources of the Self: The Making of the Modern Identity*.⁴³ Indeed, a rather dubious recent example of the ideological work of the speculative dialectic in redeeming meaning, repressing or marginalizing trauma, and recuperating loss in history is Taylor's ameliorative (perhaps unintentionally revisionist) characterization of the Nazis as marking an unsuccessful interruption of a victorious ethic of reducing suffering—an ethic Taylor sees as coming to its fullest historical fruition in a triumphant modern identity.⁴⁴

In the present context, I would like to turn briefly to John Toews' substantial, justly influential review essay, "Intellectual History after the Linguistic Turn: The Autonomy of Meaning and the Irreducibility of Experience"—an essay that concludes with a favorable appreciation of an essay by Taylor that predated *Sources of the Self*. Toews' contribution appeared at a crucial juncture in the recent history

⁴¹ See, for example, *New Directions in American Intellectual History*, John Higham and Paul K. Conkin, eds. (Baltimore, Md., 1979). Among European historians, Robert Darnton acknowledges the influence of Geertz; see *Great Cat Massacre*; and "The Symbolic Element in History," *Journal of Modern History*, 58 (1986): 218–34. For Geertz's treatment of the Balinese cockfight, see "Deep Play: Notes on a Balinese Cockfight," in *The Interpretation of Cultures* (New York, 1972). For a critique of it, see Vincent Crapanzano, "Hermes' Dilemma: The Masking of Subversion in Ethnographic Description," in *Writing Culture: The Poetics and Politics of Ethnography*, James Clifford and George E. Marcus, eds. (Berkeley, Calif., 1986), esp. 68–76. Crapanzano argues that Geertz's treatment is characterized by the absence of the native's point of view, the excessive, decontextualized stylization of performance, the transcendental perspective of the narrator, and the avoidance of a dialogic relation to the other.

⁴² For a truly masterful instance of the redemptive model, see M. H. Abrams, *Natural Supernaturalism: Tradition and Revolution in Romantic Literature* (1971; New York, 1973). For a different approach to history as the story of meaning, which is less concerned with contemporary values and more attuned to ideological conflict in the past, see Keith Michael Baker, "On the Problem of the Ideological Origins of the French Revolution," in Dominick LaCapra and Steven L. Kaplan, eds., *Modern European Intellectual History: Reappraisals and New Perspectives* (Ithaca, N.Y., 1982), 197–219.

⁴³ LaCapra, *Representing the Holocaust*, 178–87; Caroline Walker Bynum, *Holy Feast and Holy Fast* (Berkeley, Calif., 1987); Charles Taylor, *Sources of the Self* (Cambridge, Mass., 1989).

⁴⁴ Taylor, *Sources of the Self*, 575. Here, Eric Santner's notion of narrative fetishism is pertinent: "By narrative fetishism I mean the construction and deployment of narrative consciously or unconsciously designed to expunge the traces of the trauma or loss that called that narrative into being in the first place . . . [It] is the way an inability or refusal to mourn employs traumatic events; it is a strategy of undoing, in fantasy, the need for mourning by simulating a condition of intactness, typically by situating the site and origin of loss elsewhere." Santner, "History beyond the Pleasure Principle: Some Thoughts on the Representation of Trauma," in Saul Friedlander, ed., *Probing the Limits of Representation: Nazism and the "Final Solution"* (Cambridge, Mass., 1992), 144.

of the profession. Variants of the “linguistic turn” had captured the interest of historians.⁴⁵ Some historians even seemed to be appropriating aspects of contemporary critical theory in their own work, and others were looking for a way to understand and respond to these newer, or “new-fangled,” initiatives. Toews’ essay quickly became the avenue through which many historians came to understand—and react to—the so-called linguistic turn in historiography and especially the use in the profession of deconstructive and more generally poststructural thought. Moreover, Toews’ essay helped to inaugurate a significant turn in the *American Historical Review* itself in the direction of questions of theory and method as at least a necessary and valuable supplement to monographic research. The level of Toews’ discussion is consistently high, and, whether or not one agrees with all of his arguments, one must appreciate the serious thought—what Hegel in the preface to *The Phenomenology of Mind* called the *Ernst des Begriffs* (the seriousness of the concept)—that lies behind them and the critical inquiry they are able to prompt.

Toews’ basic argument is summarized in his subtitle: the autonomy of meaning and the irreducibility of experience. In accordance with his neo-Hegelian view, culture is a medium (or mediation) to make experience meaningful, and language is its primary means of accomplishing this (redemptive) feat. The engaging movement of the review essay, similar to Hegel’s *Phenomenology of Mind*, involves bringing each work under discussion to the cliff-hanging juncture of some internal difficulty, which then motivates the move to the next work, until one arrives at a penultimate point of dialectical reversal and at least a hint of the ultimate hope of speculative synthesis. The dialectical reversal indicates that not only must meaning be redeemed from experience but experience itself must be saved from meaning in a two-way repudiation of reductionism that is not completely terminal insofar as there is the hope of some form of reconciliation or integration.

Meaning, experience, and language triangulate Toews’ argument as its three-fold conceptual foundation, but their definition, relations, and history may be more problematic than Toews allows. It is, for example, significant that contemporary linguistic theories of meaning stem in good part from Ferdinand de Saussure. Saussure’s narrowly linguistic orientation is explicitly criticized by Mikhail Bakhtin and his school, who argue against its abstract, ahistorical formalism and by contrast defend a theory of language in historical use involving the problem of contextualization.⁴⁶ Foucault takes the latter understanding of language-in-use in the direction of power and institutions, while Derrida elabo-

⁴⁵ See Martin Jay, “Should Intellectual History Take a Linguistic Turn? Reflections on the Habermas-Gadamer Debate,” in LaCapra and Kaplan, *Modern European Intellectual History*, 86–110. See also the essays in *The New Cultural History*, Lynn Hunt, ed. (Berkeley, Calif., 1989). The role of a linguistic turn has also preoccupied practitioners of, and commentators on, the new historicism in literary studies. See Brook Thomas, *The New Historicism: And Other Old-Fashioned Topics* (Princeton, N.J., 1991); *The New Historicism*, H. Aram Veeser, ed. (New York, 1989); and *Introduction to Scholarship in Modern Languages and Literatures*, Joseph Gibaldi, ed., 2d edn. (New York, 1992), esp. Robert Scholes, “Canonicity and Textuality,” 138–58, and Annabel Patterson, “Historical Scholarship,” 183–200.

⁴⁶ See in particular V. N. Volosinov, *Marxism and the Philosophy of Language*, Ladislav Matejka and I. R. Titunik, trans. (New York, 1973), esp. 52–63.

rates a notion of a generalized text or a network of institutionalized traces that does not exclude reference, reality, or "experience" but attempts (however debatably at times) to situate them in a critical manner and to explore their limits. Moreover, meaning is contested internally and limited by forces that relate language to other phenomena in complex ways (for example, phonemes, drives, and bodily processes—including psychosomatic ones—that have complex relations to signification but cannot be reduced to it).⁴⁷

The most problematic term in Toews' discussion may well be "experience," for in other quarters it has become something of a scare word that intimidates opponents (those who do not share or at least empathetically understand the "experience" in question—those who have not been through it). It is also a means of authenticating one's own position or argument. Indeed, it often functions as the blackest of black boxes in general usage and perhaps to some extent in Toews' own sophisticated, well-developed argument.⁴⁸ If one means by it the common-sense notion of having lived through something, for example, with reference to the experience of the Holocaust or of rape victims, it retains an important role. But one should specify the nature of this role and inquire into its implications. For example, one may argue that experience provides a basis for a subject-position that, especially in certain cases (such as that of victims), should be respected and attended to, and it may even give a *prima facie* claim to knowledge. But experience in and of itself neither authenticates nor invalidates an argument or point of view, and it cannot be invoked to silence others—either those having or those not having it.⁴⁹ Moreover, there are many types of experience, including that of reading or writing texts, and the latter may in certain cases be quite significant. They may even be crucial components in working through other experiences or even traumas.⁵⁰

⁴⁷ The very play of material signifiers affects the body on a level undercutting signification—the level of imagination and even sensation that Julia Kristeva relates to the pre-Oedipal condition of the infant (or the *chora* in Plato). See her *Révolution du langage poétique* (Paris, 1974) (partial translation as *Revolution in Poetic Language*, Margaret Waller, trans., New York, 1984); and *Black Sun: Depression and Melancholia*, Leon S. Roudiez, trans. (New York, 1989).

⁴⁸ Joan Scott observes: "Experience for Toews is a foundational concept . . . [It] thus provides an object for historians that can be known apart from their own role as meaning makers and it then guarantees not only the objectivity of their knowledge, but their ability to persuade others of its importance. Whatever diversity and conflict may exist among them, Toews's community of historians is rendered homogeneous by its shared object (experience). But as Ellen Rooney has so effectively pointed out, this kind of homogeneity can exist only because of the exclusion of the possibility that 'historically irreducible interests divide and define . . . communities.'" "'Experience,'" in *Feminists Theorize the Political*, Judith Butler and Joan W. Scott, eds. (New York, 1992), 32.

⁴⁹ Those with extreme experience, such as rape victims, may be placed in the double bind of being authenticated as witnesses but invalidated as knowledgeable commentators concerning that experience. Thus, on talk shows, one typically has the victim as witness but the expert (for example, the psychiatrist) as the one who knows the meaning of the experience. (I thank Linda Alcoff for this insight.) This double bind is more generally at play with respect to those in subordinate or "objectified" and objectified positions, for example, "native informants."

⁵⁰ Aside from its ordinary senses, "experience" was a crucial concept for Hegel. Jean-François Lyotard observes: "The word *experience* is the word of the *Phenomenology of Mind*, the 'science of the experience of consciousness.' Experience is the 'dialectical process which consciousness executes on itself' . . . In the sphere that belongs to it, experience supposes the speculative element, the 'life of the mind' as a life which 'endures death and in death maintains its being' . . . This abode liberates the *Zauberkräft* [magical force] of the mind, the power to convert the negative into Being, the '*göttliche Natur des Sprechens*' [divine nature of speech]." Lyotard, *Differend*, 88–89.

One should not peremptorily dismiss the concept of experience or the need to come to terms with it, and Toews' article is extremely helpful in prompting significant questions about it. One of the merits of his complicated account is to introduce unexpected intricacies into one's conception of "experience" and to indicate how a contemporary development of Hegel's thought—or even a fair appreciation of it—would resist a facile tendency to reject all constructive, substantively rational activity as redemptive or totalizing. But three further points may be made concerning the way this concept may function to exclude crucial possibilities. One concerns the role of trauma that escapes experience in the ordinary sense and upsets the movement of a harmonizing or synthesizing speculative dialectic. The "significance" of trauma is that it disrupts experience and cannot be integrated into it. For Freud, trauma cannot be located punctually but takes place belatedly (*nachträglich*) as a later, seemingly insignificant "experience" somehow recalls an earlier one, charged but unassimilated. The trauma in the technical, psychoanalytic sense is marked by an interval between experiences, and it involves a period of latency between the initial and triggering "experiences." More generally, trauma does not conform to either a phenomenological or a common-sense model of experience, and it is typically repressed or denied in a manner that induces its compulsive reenactment and the need to work through it. Rather than repressing or excessively mitigating its role (as in Taylor's *Sources of the Self*), a critical historiography would allow trauma to register in the (perhaps never fully successful) attempt to work it through. Moreover, it may, paradoxically, be the case that the only "full," prepossessing experience is phantasmatic, and its relation to meaning is problematic: the experience of compulsively acting out a (real and/or imagined) traumatic past that is relived as if it were entirely present (rather than remembered with the risk of gaps and other difficulties that memory brings). One may also insist that working through trauma does not deliver full meaning or speculative synthesis but instead permits a significant measure of critical control that may never entirely—at least in cases of severe trauma—dispense with at least the possibility (and in all probability the reality) of acting out.⁵¹

A second point is that one of the main purposes of history is to inquire into the significance of what one did *not* experience and what one reconstructs on the basis of multiple remains, including at best the traces of others' experiences and traumas. In this sense, one cannot simply ground history in experience, and the

⁵¹ For a discussion of the way the concept of experience has been used in postwar Germany to deny, repress, or mitigate trauma, see Eric L. Santner, *Stranded Objects: Mourning, Memory, and Film in Postwar Germany* (Ithaca, N.Y., 1990), esp. the analysis on 89–97. On trauma, see also my *Representing the Holocaust*; and "European Intellectual History and the Post-traumatic State," an interview in *iichiko Intercultural*, no. 6 (1994): 108–26. In his sensitive, informative book, *The Texture of Memory: Holocaust Memorials and Meaning* (New Haven, Conn., 1993), James E. Young writes of concentration camps as memorials and specifically of Majdanek: "The ruins here are material evidence not only of these crimes [against Jews and other victims of the Shoah] but also of a state's reasons for remembering them. Indeed, there is little reason for preserving the ruins outside of the meaning preservation imputes to them" (p. 121). One may qualify Young's assertions by noting that memorial ruins may also mark trauma and delimit meaning. Besides being *lieux de mémoire* (in Pierre Nora's term), they are also *lieux de trauma*, that is, sites for acting out problems that resist being imbued with satisfactory meaning and that may not be adequately remembered or worked through. One may also note that despite the ideological individualism and adamant rejection of psychoanalysis as applied to collective phenomena in his preface (xi), Young, in his substantive discussions of the role of memorials in various social contexts, makes frequent appeals to psychoanalytic concepts such as repression.

problem is not to guard against the reduction of experience to meaning but to enable oneself and others to understand and remember what may be distant from one's own experience or that of one's community (or what one takes that experience to be). How understanding and memory should work is debatable, for example, in terms of the role and relative importance of empathy, the interpretation of meaning, and the active recognition of the limits of both empathy and meaning. But that history extends beyond (or falls short of) experience, without simply providing either full meaning or deceptively vicarious experience, should be evident.

A third consideration is that a prevalent characteristic of "postmodern" culture is the commodification of experience, and the invasiveness of this process may raise doubts about the uncritical invocation of the term. We now buy and sell experiences and not simply goods and services—the experience of visiting an Indian reservation, of getting to know Santa Fe, of living for a while in a monastery, of attending a university such as Harvard or Yale, or even of spending an afternoon in a Holocaust museum participating in certain "experiences" of victims. With the advent of virtual reality, we now can market simulated experiences detached from their "real" referents. Indeed, we try to escape from commodification through a phenomenon that is readily fed back into the commodity loop through soap operas and popular literature: the meaningful experience itself. The meaningful experience does not reduce experience to meaning (the move Toews fears); it joins the two in a blissful *Aufhebung*, which shows how Hegel, too, can be conventionalized, provide one facile way to shoot the gap between high and mass culture, and be made to render good service for contemporary life. ("His pain, our gain," as one postmodern, Christological T-shirt has it.)

5. Dialogic Reading. Since I have attempted in my own work to develop and apply a dialogic approach to reading and interpretation, I shall tend in this section to speak more fully in my own voice. Yet here, a strong caveat is in order. "Dialogue" is itself a contemporary "buzz word" that is like "experience" in that it, too, has been thoroughly commodified if not banalized. And it is difficult in discussing an approach one has advocated to avoid an upbeat Hollywood ending that strikes a redemptive note. For these reasons, it is important to distinguish dialogism from dialogue in the ordinary, banalized sense. Dialogism refers in a dual fashion both to the mutually challenging or contestatory interplay of forces in language and to the comparable interaction between social agents in various specific historical contexts. (Especially in the former sense, its concerns parallel those of a certain mode of deconstruction.) Basic to it is a power of provocation or an exchange that has the effect of testing assumptions, legitimating those that stand its critical test and preparing others for change. (This is, I think, the crucial sense of dialogism in Bakhtin that involves the role of the carnivalesque.) Moreover, in my understanding of it, a dialogic approach is based on a distinction that may be problematic in certain cases but is nonetheless important to formulate

and explore. This is the distinction between accurate reconstruction of an object of study and exchange with that object as well as with other inquirers into it. This distinction itself indicates that there are limits to dialogism that prevent it from achieving the status of a redemptive or totalizing perspective. It is both necessarily supplemented by other perspectives, such as reconstruction, and questioned by forces it does not entirely master (such as differences in power, the effects of trauma, or the workings of the unconscious).

History in accordance with a self-sufficient research paradigm gives priority if not exclusive status to accurate reconstruction, restricts exchange with other inquirers to a subordinate, instrumental status (signaled textually by a relegation to footnotes or a bibliography), and is forced to disguise dialogic exchange as reconstruction, often in a manner that infiltrates values into a seemingly objective or value-neutral account. It is less deceptive to argue that one may make a problematic yet significant distinction between reconstruction and exchange and that exchange is permissible—indeed, both unavoidable and desirable—for the historian, with respect both to the object of inquiry and to other inquirers.

The distinction between reconstruction and exchange does not imply the feasibility of a binary opposition or separation of the two into autonomous activities or spheres. Exchange with other inquirers is constitutive of research, for it helps to shape the very questions one poses to the past and establishes a contemporary context (typically involving ideological issues) that should be critically elucidated rather than occluded, repressed, or relegated to a secondary position. In this sense, there is a mutually reciprocal relation between research and dialogic exchange, for the object of research is constructed in and through exchange with past and present inquirers. In addition, exchange with the object of inquiry (which is always mediated by exchange with other inquirers) is necessary, notably with respect to intensely “cathected” or traumatizing objects, such as the Holocaust, or texts that themselves raise problems of continuing concern and demand a response from the reader not restricted to purely empirical-analytic inquiry or contextualization.

A dialogic approach involves the recognition that projection is to some extent unavoidable insofar as objects of inquiry are of intense concern to us because they pose questions that address significant values or assumptions. At times, they may pose such questions precisely because they differ drastically from what we hold, or would like to believe we hold, our basic values or assumptions to be. This point applies both to the beliefs and practices of very different cultures or time periods and to more recent phenomena that upset cherished convictions about the nature of our civilization. The difficulty in coming to terms with the Nazi period and the desire to normalize it in one way or another, if only by showing the prevalence of genocide or, on the contrary, the manner in which it was presumably anomalous or marginal in German or Western history, attest to the manner in which we tend to refuse to see that it was indeed a real possibility for an important part of “our” civilization and thus for “us” under certain conditions. An obvious but basic point is that something would not shock us if it were not already in us, in however potential, subdominant, or repressed a form. If it were not already in us, it would not provoke anxiety but simply leave us indifferent as an object of idle curiosity.

The very incredulity evinced by the fact that the Shoah could have happened in the land of Goethe and Kant is remarkable both because of its naïveté and because of the manner in which it signals the occurrence of a shock or trauma we find difficult to assimilate.

A combination of accurate reconstruction and dialogic exchange is necessary in that it accords an important place to the “voices” and specific situations of others at the same time as it creates a place for our “voices” in an attempt to come to terms with the past in a manner that has implications for the present and future. It is in this sense that it remains important to provide quotations from a text being interpreted or from agents in the period being discussed. The principle here is that such quotations should be extensive enough to provide the reader with a basis for a possible counter-reading or interpretation in the event that the latter is indeed called for. In reading a text, one may formulate the combination of reconstruction and dialogic exchange most simply in terms of two related questions: What is the other saying? How do I—or we—respond to it?

This formulation is simplistic in that it does not address the complexity of determining what the other is saying or explicate the divergent possibilities of response. To arrive at what the other is saying requires some determination of literal and figurative meaning, the role of irony, parody, and “voice” or positionality in general, the possible intervention of unconscious forces such as repression and denial, and the articulation of undeveloped potentials of the text or utterance. It also requires sensitivity to the projective dimensions of our attempts at reconstruction that become more insistent to the extent that the object of inquiry is still highly charged for us. Here, there is the possibility of a postdeconstructive notion of objectivity that supplements rather than obviates the role of dialogic exchange: a notion of objectivity in which we attempt to check projection and prevent it from becoming a unilateral if not narcissistic reprocessing or monologic rewriting of the phenomenon or text. Instead, we employ contextualizing techniques, requiring meticulous research and the attempt to substantiate statements, precisely as checks on projection. Deconstruction may signal points at which the attempt at dialogic exchange is blocked because the traumatic aspect of the text or phenomenon is so great that it makes exchange and perhaps language in general break down in a more or less telling way—a possibility that entails not the futility of all dialogue but the recognition of its limits. Moreover, radical deconstruction tends to stay within trauma and to act it out or perform it; and this procedure, while to some extent necessary in the face of traumatic crises or limit-cases, is especially misleading when it is autonomized and does not broach the question of how to work through problems. Still, deconstruction can be of value in bringing out the internal tensions, contradictions, and aporias of texts or phenomena, and such inner contestations may well indicate problems that were not—and may still not be—acceptably thought and worked through. In certain of its forms (notably in Derrida’s writings), it may also bring out the significance of play and laughter as well as their possible role in working through problems. But a dialogic approach does not postulate an antinomy between reading and interpretation, hermeneutics and poetics, work and play. Rather, it takes those relations to be problematic as it investigates the

possibility and limits of meaning in the past in its bearing on the present and future. Here, the attempt at reconstruction itself broaches the question of how to engage in dialogic exchange.

Dialogic exchange indicates how the basic problems in reading and interpretation may ultimately be normative and require a direct engagement with normative issues that are often concealed or allusively embedded in a seemingly "objective" account. It also brings out the problem of the relations among historiography, ethics, and politics. For the dialogic dimension of inquiry complicates the research paradigm and confronts one with the problem of the voice and subject-position(s) in which one responds to the past in a manner that always has implications for the present and future. The use of the "I" is relatively uncommon in historiography and is often restricted to a preface or coda. It is becoming more common, perhaps too common, in literary criticism and anthropology. Its use is, in any case, ambivalent or even equivocal. It disrupts a value-neutral façade and raises questions about the possibilities and limits of objectivity. It foregrounds the problem of subjectivity. But it easily reinforces an individualistic ideology, obscures the problem of subject-positions, and may foreclose other possible responses, such as collective and more politically germane ones. The notion of subject-position signals the intimate relation of subject and subjectivity to social positionality and the manner in which "voice" is not a purely individual or subjective issue. It also brings "voice" in contact with ethical and political issues that are not confined to the individual psyche or biography. It thus forces the issue of the nature of a desirable dialogic response to the past.

The dialogic dimension of research in which a response is required of the investigator has been acknowledged in a restricted manner in the notion of observer-participation. This dimension heightens awareness of, even anxieties about, the historian's interactions with the object of inquiry. It is thus worth returning to the proposal that one consider these transactions in psychoanalytic terms—as involving a transference relation to the object of study (as well as to other inquirers). Transference in Freud rested on the tendency to repeat, either in a compulsive form of acting out (in which one relived a typically traumatic past as if it were fully present) or in a more critically controlled "working-through" that allowed for significant change and a reinvestment in life. The transference repetition in the clinical context of the relationship between parent and child was seen by Freud as occurring in less controlled or safe environments such as the adult romantic or even the work relationship. Even in its restricted, orthodox Freudian sense, transference may have a bearing on the relation among scholars (notably the teacher–graduate student relationship) that has yet to be sufficiently acknowledged and accounted for.⁵² But the more general and basic sense of transference as a tendency to repeat applies as well to the way in which processes active in the object of study tend to be replicated in our accounts of them. This transference relation in the broad sense requires that we come to terms with it in one way or another—through denial (as in positivism or notions of pure objectivity), repression (in research that brackets or marginalizes values, only to

⁵² On this issue, see Peter Loewenberg, *Decoding the Past: The Psychohistorical Approach* (New York, 1983), 45–80.

see them return in encrypted or covert form), acting out (as in certain views of performativity or active rewriting), or working-through (the goal of a critically controlled dialogic exchange with the past). Indeed, insofar as the fundamental concepts of psychoanalysis (such as transference, repression, denial, resistance, acting out, and working-through) are not restricted to the individual psyche or the one-on-one clinical situation but recognized as undercutting the opposition between the individual and society and linked with the notion of subject-position, they provide a way of rethinking the problem of reading and interpretation in history. They may even furnish the basis for developing an ethics of reading that is not insensitive to the role of play, laughter, and carnivalesque forces in general.

IN CONCLUDING, I would reiterate the basic point that different historians may justifiably embody in their work a different combination of reconstruction of the object, involving contextualization in terms of the past, and dialogic exchange with it that itself calls for self-contextualization in a present network of discussion and debate. Indeed, different works or portions of a work by the same historian may show differing stresses and strains. This view considerably broadens the field of history without depriving it of all coherence. Rather, it introduces into it the need for informed argument about boundaries and the recognition of how certain forms of testing—or even periodic transgressing—of those boundaries may be fruitful for the very self-understanding of historians and the reconsideration of disciplinary definitions. Finally, it also raises the question of what combinations of subject-positions should be deemed allowable or desirable even when they involve the passage beyond a delimited disciplinary conception of the historical profession.

Revisioning U.S. Political History

MARK H. LEFF

IT IS THE LOT OF ACADEMICS to feel unappreciated. But some academics are more unappreciated than others, or so many political historians will tell you. By the 1970s, something had gone terribly wrong. Jobs, grants, and prestigious publications increasingly went to “social science types,” especially social historians who disdained political history as elitist, shallow, altogether passé, and irrelevant to the drama of everyday lives. “The old ‘presidential synthesis,’” Eric Foner declared, “is dead (and not lamented).”¹

The “political history” of the United States, as Foner of course fully recognized, has always involved more than a study of presidential administrations. In some hands, in fact, it can extend to any power relationship—from the family to the shop floor to the shopping mall—or to any conflict over “public space.” But such sprawling definitions of political history have little to do with the actual work of people who call themselves political historians. So, for the purposes of this article, I propose a working definition designed less for rigor than to reflect common practice: political history deals with the development and impact of governmental institutions, along with the proximate influences on their actions.

Even by this delimited definition, political history has remained the major concern of the *American Historical Review* for most of its first hundred years. In the context of that history, the very appearance of this article might be taken as another indication of political history’s decline. Review articles, after all, are customarily devoted to confined chronological periods, to particular themes, or to so-called subfields such as American medical, intellectual, legal, or labor history—not to a field that many had once considered synonymous with U.S. history itself.²

Yet decline is not the subject of this article; if the truth be told, my vantage point is somewhat Whiggish. I see this one hundredth anniversary of the *AHR* as a propitious time to consider the development of political history. Two paths beckon. On the surface, both might offer political historians reason for optimism. But one path toward enhanced influence, I would argue, has real promise, while

I have benefited greatly from critical (sometimes very critical) readings by James Barrett, Carol Skalnik Leff, Alan Brinkley, Douglas Greenberg, Barry Riccio, Jamil Zainaldin, Paul Schroeder, and the Social History Group of the Department of History, University of Illinois, Urbana-Champaign. I am also much beholden to the Program for the Study of Cultural Values and Ethics at the University of Illinois for its invaluable support during my fellowship period.

¹ Eric Foner, ed., *The New American History* (Philadelphia, 1990), ix.

² As recently as the 1980s, for example, the omission of political history from the field reviews in the American Historical Association’s *New American History* and *Reviews in American History*, 10 (December 1982), could be taken as a backhanded tribute to its scope.

the other directs historians into a blind alley. The first opportunity is an invitation to political historians to remove the blinders that limited their vision in the early years of the *AHR*, leaving much of the American historical experience hidden from view. This re-visioning of American political history, through cross-fertilization with related fields, provides a far better opportunity than earlier, constricted views to meet political historians' current shared civic and educational goals. In the process, such a re-visioning enhances the profession's coherence and opens the American historical experience to unprecedentedly broad and diverse audiences. The second path to potential influence, marked by nostalgic calls for a return to something labeled "traditional" political history, has been blazed by politically motivated attacks, from inside and outside the profession, on recent historiographic trends. On this path, political historians can attempt to recapture their former professional dominance by reversing and inhibiting the revisionist interpretive process, returning to a more celebratory history that privileges leadership achievements and virtues. Many educated Americans today—schooled in a simplified textbook-styled U.S. history—still see this as *the* American story. But this path, while serving the political interests of today's critics, would undermine political history's integrative potential for the profession, isolate it from the larger discipline, and extinguish history's current claims to educational importance, reducing the historian's public role to a dutiful echo of popular conventional wisdom. As American Historical Association president John Coatsworth asks, "why hire professional historians and curators to do an honest, thoughtful job when you really want propaganda?"³

Looking back through the lenses of the embattled present, we might see history as practiced in the founding years of the *AHR* as political historians' "good old days," when their field appeared to be virtually the only valid terrain of historical inquiry. Certainly, the linkage between politics and history, or more precisely between the study of politics and the study of history, was both prominent and celebrated. Combined history and political science departments were commonplace.⁴ "History entered academic curricula," David Van Tassel points out, "as an approach to political science."⁵ When Columbia University's John W. Burgess proclaimed in the *AHR* that history and political science "so lap over one another and interpenetrate each other that they cannot be distinctly separated," dissident historians objected less to the sentiment than to the prospect that political scientists were imperialistically eyeing their turf.⁶ Herbert Baxter Adams, the secretary of the AHA and the key figure in its founding, dedicated his famous Johns Hopkins seminar to Edward Freeman's dictum that "History is past Politics and Politics present History"—a motto that he reportedly cited so often that some "began to think that Adams had written it himself."⁷ Even younger scholars,

³ John H. Coatsworth, "Letter to the Editor," *Wall Street Journal* (February 28, 1995): A15.

⁴ David Brian Robertson, "History, Behavioralism, and the Return to Institutionalism in American Political Science," in Eric H. Monkkenen, ed., *Engaging the Past: The Uses of History across the Social Sciences* (Durham, N.C., 1994), 114–15.

⁵ David D. Van Tassel, "From Learned Society to Professional Organization: The American Historical Association, 1884–1900," *AHR*, 89 (October 1984): 943.

⁶ John W. Burgess, "Political Science and History," *AHR*, 2 (April 1897): 408; Dorothy Ross, *The Origins of American Social Science* (Cambridge, 1991), 266.

⁷ Philip Etherington and Eileen McDonagh, "Introduction" to "Conference Panel: The Intellec-

increasingly inclined to assert that a “detailed contextual reconstruction of the past” necessitated a self-conscious specialization in history and a recognition of the “difference of the past,” bore the imprint of this “historicopolitical” approach.⁸

“Institutional history,” primarily the study of political institutions, was the centerpiece of this approach; *AHR* editor John Franklin Jameson was adamant about this, even while he was already lecturing by 1895 on “The American Revolution as a Social Movement.”⁹ Moreover, Dorothy Ross notes, most historians at the end of the nineteenth century embraced the “task of finding in history viable principles for current political action.”¹⁰ This politics possessed definite ground rules and boundaries that determined who and what deserved attention. In the early years of the profession, when much of the rest of the nation was in turmoil, most professional historians were, in Peter Novick’s only mildly overstated assessment, “serene and untroubled in their celebration of traditional pieties: an island of orthodoxy in a sea of heterodoxy.”¹¹

This uniformity had a great deal to do with the homogeneous character of the profession itself. The first issue of the *AHR* opened with a statement of principles, “History and Democracy,” written by a member of its Board of Editors, William Sloane. Sloane, an intellectual leader in the profession and a future president of the AHA, was very clear on the new journal’s audience. “We are Europeans of ancient stock,” he noted. “[W]e brought with us from England, Scotland, Ireland, Holland, Germany, and France” a “well-ordered, serious life” and created “a set of distinctively American institutions” that had been extended to immigrants and to former slaves.¹² The very next article in the *AHR*, an early entrant in the Imperial School of analysis of the American Revolution (which found the Loyalists’ emphasis on stability, order, and law decidedly more attractive than the rabble-rousing of a popular uprising), characterized its readers as “hereditary partisans” whose “fathers and grandfathers” had errantly derided Loyalist arguments.¹³ Its author, Moses Coit Tyler, went on to describe the Loyalists in almost precisely the terms that many *AHR* readers would use to describe themselves. “Will it be denied,” Tyler asked, that most Loyalists were “conservative people” and that, “within that order of persons, one may usually find at least a fair portion of the cultivation, of the moral thoughtfulness, of the personal purity and honor, existing in the entire community to which they happen to belong?”¹⁴

tual Legacy of the Johns Hopkins Seminary of History and Politics; Reconsidering the Genealogy of the Social Sciences,” *Studies in American Political Development*, 8 (Fall 1994): 380; Joan Wallach Scott, “History in Crisis? The Others’ Side of the Story,” *AHR*, 94 (June 1989): 680. Scott characterized this as “the founding motto of the American Historical Association.”

⁸ Dorothy Ross, in “Conference Panel,” 391–93.

⁹ Ross, *Origins of American Social Science*, 268. Jameson’s lifelong skepticism toward other methodologies is discussed in Morey D. Rothberg, “‘To Set a Standard of Workmanship and Compel Men to Conform to It’: John Franklin Jameson as Editor of the *American Historical Review*,” *AHR*, 89 (October 1984): 967.

¹⁰ Ross, *Origins of American Social Science*, 266.

¹¹ Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (Cambridge, 1988), 63.

¹² William M. Sloane, “History and Democracy,” *AHR*, 1 (October 1895): 15.

¹³ Moses Coit Tyler, “The Party of the Loyalists in the American Revolution,” *AHR*, 1 (October 1895): 26–27.

¹⁴ Tyler, “Party of the Loyalists,” 30.

In the several fine scholarly commentaries on the profession's formation, there is ready agreement that the profession's founding fathers defined themselves and their immediate audiences as gentlemen, as a genteel intellectual and social elite. Even low-salaried professors found that "their European training, dedication to learning, and high culture gave them entrée to elite circles in the United States."¹⁵ The "cultivated patrician class," notes John Higham, played a vital supportive role in organizing the profession. They joined with professional scholars as "a homogeneous class with a common mission": to rein in the democratic excesses that so repelled them, and to employ the privileges of breeding and knowledge to assume a leadership role in uplifting American culture, values, and—perhaps most of all—public life and political debate.¹⁶

Given this mission, derived in part from the same alarm over uncontrolled immigration and industrial change that spawned patriotic societies, preservation associations, and other forms of "ancestor worship," the historical profession became part of what Michael Kammen terms "the Party of Memory."¹⁷ The institutional history that anchored the profession was meant to serve as a vital unifying and ordering force, directed not so much at the broader population (that target, in this new professional world, was often left to the dramatic flourishes and "mere narrative" of popularized biographies) but to the trained leadership corps of future statesmen, professionals, and upper-level bureaucrats being molded in their classrooms.¹⁸

The objectives of that institutional history—the tricky task of "legitimizing the social and economic order" while using it to mandate a cleansing and reordering of modern life—comes through clearly in the *AHR*'s inaugural issue.¹⁹ Sloane, for example, contrasted the radicalism of European democracy—which, if unchecked, would bring about "anarchy and ruin" and "destroy all greatness both in the making and in the writing of history"—with the "orderly modern democracy" of what he called "English America"—which, thanks to the "modesty of long experience," could "furnish abundant room for the play of talent, if it exists, either in the practical statesmanship of its own age, or in the investigation of the states and statesmen of other ages."²⁰ But, to appreciate that legacy, he emphasized, "we must not go too far in yielding to a popular clamor, nor admit that the weight of the individual in modern life entitles his occupations and beliefs to more than a certain moderate share in the story of the organism to which he belongs." Citizens' interactions with the politics of the nation had their place, but those writing "real history," he counseled, "will keep the emphasis on the state and on the organs by which it nourishes and prolongs its life" and "on its conduct in peace and war and the principles which guide it," for "the conduct of the state,

¹⁵ Van Tassel, "From Learned Society," 930.

¹⁶ John Higham, *History: Professional Scholarship in America* (1965; Baltimore, Md., 1983), 8.

¹⁷ Michael Kammen, *Mystic Chords of Memory: The Transformation of Tradition in American Culture* (New York, 1991), 220.

¹⁸ Higham, *History*, 97, 158.

¹⁹ Novick, *That Noble Dream*, 61.

²⁰ Sloane, "History and Democracy," 9–10, 16.

which expresses the resultant life of those who compose it, is the essential matter of history.”²¹

Even in this period, the *AHR* was never quite as monochromatic as is implied by this focus on what we have come to think of as high political history. Politics itself was defined in a way that could encompass, as a subordinate theme, the social forces behind political “progress.” Sloane’s often brilliant and subtle *AHR* essay deprecated histories that focused entirely on great men or great speeches. “Mere political history,” he argued, must give way to more comprehensive historical study that considered the “social, industrial, commercial, aesthetic, religious, and moral conditions of the common man,” recognized how these conditions interacted with politics, and responded to contemporary standpoints and questions.²² Volume 1 of the *AHR* included an article on the Underground Railroad based largely on interviews and questionnaires of surviving participants—a pathbreaking if overly credulous study that became the standard, not superseded until Larry Gara’s book *The Liberty Line* in 1961.²³ Nor was the journal always as stuffy as quoted statements might make it seem. Charles Francis Adams provided a delightful narrative of the battle of Bunker Hill in which he characterized General Thomas Gage as “that not uncommon type of soldier familiarly known in military parlance as a ‘butt-head,’” and explained the patriot victory in hardly celebratory terms as the result of stupidity, pure luck, and a balancing of blunders on both sides.²⁴

In general, however, the historian’s story of America was primarily a sacred story with strong nationalist overtones.²⁵ This story could include tributes to the overall contributions of assimilated immigrants, but it derived much of its coherence from the groups it ignored or dismissed. Two textbook writers in 1910, reflecting this well-established approach, assured themselves that there was no need to allot more than 170 words to the Indians because they had already told of the “effect of the Indians wherever their existence changes the forward movement of the white man.”²⁶ *AHR* editor Jameson found, in the words of his biographer Morey Rothberg, that his “ambition to write the social history of America was thwarted by his obvious distaste for people in the mass and for ethnic groups other than his own.”²⁷ Under the circumstances, it was scarcely surprising that an *AHR* book review could refer matter-of-factly to “the tremendous mistake of indiscriminate negro suffrage, with all its inevitable evil results,” while William Sloane could describe “certain great cities” as “swamped . . . by the sludge thrown upon our shores from the governments of Europe, in the shape of the shiftless, stupid, and,

²¹ Sloane, “History and Democracy,” 8–9.

²² Sloane, “History and Democracy,” 5–6.

²³ Wilbur H. Siebert, “Light on the Underground Railroad,” *AHR*, 1 (April 1896): 455–63; Larry Gara, *The Liberty Line: The Legend of the Underground Railroad* (Lexington, Ky., 1961).

²⁴ Charles Francis Adams, “The Battle of Bunker Hill,” *AHR*, 1 (April 1896): 407, 410.

²⁵ See David Noble’s perceptive review of *John Franklin Jameson and the Development of Humanistic Scholarship in America*, in *Journal of American History*, 81 (September 1994): 727–28.

²⁶ Kammen, *Mystic Chords of Memory*, 258–59.

²⁷ Morey Rothberg and Jacqueline Goggin, eds., *John Franklin Jameson and the Development of Humanistic Scholarship in America* (Athens, Ga., 1993–), xxx.

too often, criminal elements of their populations.”²⁸ In a profession whose fundamental values underwent so little questioning, such sentiments passed without notice. As Lawrence W. Levine has observed, “for the most part, these earlier historians were concerned overwhelmingly with a decided minority of the population in terms of class, ethnicity, region, and gender, and tended to confuse the history of one group with the history of the nation.”²⁹

THUS, EVEN FOR POLITICAL HISTORIANS, this anniversary of the *American Historical Review* provides a flawed model for reinstatement. Better, perhaps, to cast down their bucket where they are and exploit current critiques of historical practice in order to extend their influence. These critiques proceed at two intersecting levels. The first comes from within the profession, from historians who ruefully note that the dethronement of political history has brought overspecialization, a dissolution of the essential unity of the American story, and isolation from broader publics. According to what has become an almost mantra-like self-criticism, historians speak only to ever more fragmented professional subgroups, marginalizing themselves by the yawning gap between their message and methodologies on the one hand and what an alienated public on the other hand can understand or is willing to hear. The second critique reverberates through the public arena, where the profession is taken to task for “political correctness.”

Can political history, whose decline is implicated as part of the problem, emerge in a reintegrative role that can be part of the solution? Many postmodern scholars think not. To them, any “totalizing” “master narrative” of American history, political or not, is suspect. They join a range of historians in arguing that the underlying periodizations and frameworks of U.S. history rely too heavily on political events as it is, particularly for the twentieth century. Any history that takes political life as its starting point, many suspect, will remarginalize the very groups that have only recently found a secure place in the historical spotlight, robbing them of their internal identities in order to view them in interaction with more powerful groups.³⁰

These are legitimate concerns, reflected in recent debates among historians over whether the public sphere is best viewed as an arena for the contested visions of diverse groups, as a magnet that draws on and reinforces common patterns of experience, or as a forum for exploring the relationships between unity and

²⁸ Book review by Harry Pratt Judson, in *AHR*, 1 (April 1896): 564; Sloane, “History and Democracy,” 11–12.

²⁹ Lawrence W. Levine, *The Unpredictable Past: Explorations in American Cultural History* (New York, 1993), 7.

³⁰ For the emphasis on political history in twentieth-century U.S. history, see Alan Brinkley, “Writing the History of Contemporary America: Dilemmas and Challenges,” *Daedalus*, 113 (Summer 1984): 124–25. For arguments that the very notion of a transcendent civic public banishes the interests and perspectives of oppressed groups by denigrating them as private or particularized, see Sara M. Evans, “Women’s History and Political Theory: Toward a Feminist Approach to Public Life,” in Nancy A. Hewitt and Suzanne Lebsock, eds., *Visible Women: New Essays on American Activism* (Urbana, Ill., 1993), 123, 129, 139; Iris Marion Young, *Throwing Like a Girl and Other Essays in Feminist Philosophy and Social Theory* (Bloomington, Ind., 1990), 94–110; Scott, “History in Crisis?” 691.

diversity.³¹ Yet even these debates over the character of the public sphere rest on a common assumption that suggests the potential for a resurgent role for political history: a recognition that interactions in the public sphere are a critical part of the life of a nation and of the individuals within it. Government and the public sphere have actually mattered in people's lives. This is a truism so self-evident that some have practically shrugged it off—even as it was confirmed, with far more forbearance and grace than most of us can muster, by William E. Leuchtenburg in his presidential addresses to the Organization of American Historians and the AHA.³²

Similarly, a historical understanding of politics is so fundamental to many of the most basic shared goals for history education that it may seem superfluous even to recall them. Few can deny its potential role in promoting such essentials of democratic civic life as the ability to challenge preconceptions, to engage in free critical inquiry, and to participate in collective decision making through reasoned, informed argument; the capacity to analyze vital public-policy issues and to assess the impact of past efforts to confront them; a tolerance for and empathic understanding of different points of view; an appreciation of common political ideals, contested alternatives, and the limits to their realization; and a shared vocabulary and set of contextually grounded understandings that can promote community ties while allowing for enlightened public debate and action.

It is, in fact, these shared goals, the sense that history has a vital public role, that lends such urgency to historians' self-criticism. Some historical perspective may be instructive here, however. It is worthwhile to keep in mind that such jeremiads on historians' insularity are "old and chronic."³³ The professionalization process works in not-so-mysterious ways. As emerging subfields and methodologies dilute the dominance of old ones, cries of overspecialization by those who feel displaced are inevitable.³⁴ Expert knowledge is also taken as its own justification for a broader public hearing. Unfortunately, despite calls for American historians to "reclaim a general public," it is hard to locate the time when they staked the original claim.³⁵ Certainly, the founding years of the *AHR* suggest no such utopia; even the leading "popular" historians who had not turned inward toward fellow professionals usually found sales limited to the low thousands. Reading "respectable" history at the turn of the century was more a gentleman's occupation notable for what Henry Adams termed "its social distinction," what has been labeled a "class marker."³⁶

Without denying the obvious—that broader audiences are a worthy goal and that the profession's incentive structure has yielded too many impenetrable and

³¹ John Higham, "The Future of American History," *Journal of American History*, 80 (March 1994): 1305; Thomas Bender, "'Venturesome and Cautious': American History in the 1990s," *Journal of American History*, 81 (December 1994): 994.

³² William E. Leuchtenburg, "The Pertinence of Political History: Reflections on the Significance of the State in America," *Journal of American History*, 73 (December 1986): 585–600; Leuchtenburg, "The Historian and the Public Realm," *AHR*, 97 (February 1992): 1–18.

³³ Higham, *History*, 68.

³⁴ Scott, "History in Crisis?" 686.

³⁵ C. Vann Woodward, quoted in Levine, *Unpredictable Past*, 7.

³⁶ Higham, *History*, 70. Paula Baker provides a broader analysis in "The Fragmentation of the Profession and Its Class Culture," *Journal of American History*, 81 (December 1994): 1150.

boring books—it may be more plausible to argue that the chief threat to professional historians today may be traced to the expansion of their audience, not its contraction. “As we finally expand our definition of historian to include those who work outside the academy and in media other than printed works,” writes Lawrence Levine, “contemporary historians are reaching out to large audiences more dramatically, more effectively, and more synthetically today than they ever did in the past.”³⁷ As the historiography itself broadens, historians are attracting new audiences who only now are finding themselves included in historical accounts. Undergraduate class enrollments, which have rebounded in recent years, are substantial enough in themselves to assure that historians reach vastly larger audiences than in the profession’s “good old days.” The tens of millions of visitors to historical museums and National Park Service historic sites are today far more likely than ever before to encounter newer work by professional historians. Evidence of popular historical interest includes popular prize-winning television documentaries such as *Eyes on the Prize*, *The Civil War*, or the “American Experience” series; numerous historical films; a large historical preservation movement; the wide adoption of Women’s History and Afro-American History Months; community and school programs, oral histories, local histories, lectures, chatauquas, and interpretive exhibits sponsored by state humanities councils; historical reenactments; a new 24-hour cable TV “History Channel”; and—yes—even best sellers by the likes of Doris Kearns Goodwin, Taylor Branch, and David McCullough. Professional historians, of course, cannot arrogate full credit for these developments, nor do they always wish to. Some, while recognizing the broad shift from antiquarian to more interpretive uses of history, complain that popular historical presentations too often sacrifice deeper contexts and conflicting interpretations to surface emotion or to inside tales of the rich and famous that bear little discernible relation to historical method. But numerous professional historians have been involved, either directly or as consultants, in many of the public history programs, and they report that “serious” scholarship has benefited greatly from participation in a public dialogue. However one assesses the strengths and weaknesses of the history that is reaching the public both inside and outside the university, the impact of that history deserves serious scholarly attention.

And attention has not been lacking; often, it appears in highly hostile form. Sometimes, the attack has come from within the academy, in debates that have been rehearsed in this journal. Assaults on rampant multiculturalism and the marginalization of political history spearhead this attack, much to the bemusement of political historians such as myself, who suspect that we are being used as a battering ram in a political assault masquerading as a methodological one.

Indeed, the political considerations that raise the stakes in this battle *on all sides* should not be ignored. As previously neglected groups come into focus on the historian’s viewing screen, there *are* unmistakable political consequences: it *can* become more difficult to sustain central themes of moral superiority and “the belief in equality and freedom” without treating them as a problematic; the

³⁷ Lawrence W. Levine, “Clio, Canons, and Culture,” *Journal of American History*, 80 (December 1993): 864.

“former heroes of political history” *can* “lose some of their democratic status” in any effort to pursue themes of American democratic aspirations in the everyday lives of excluded peoples; and any attention accorded these groups *can* imply that they have legitimate interests worthy of fuller attention.³⁸ It is, after all, inconvenient to deal with a multicultural past if one would rather orient current politics away from the reality of an increasingly diverse citizenry and the acceptance of a multicultural future; critics of history’s recent direction have both political and methodological reasons to favor a kind of “royalty test” for the profession, to assure that leaders stay at the center of the main story as the embodiment of the American people (an ironic assumption in today’s political climate).

The “new” social history, charges Gertrude Himmelfarb, a leading defender of older approaches to political history, “fails to capture the dynamic movement of history,” joins with “the prevalent ideological bias [to dispose] the historian to identify with his subjects and endow them with his own attitudes and values,” and impugns “the dignity of the people by dwelling on the least dignified aspects of their history.” By “asking questions of the past which the past did not ask of itself” and then pursuing them with “unreliable” evidence, she argues, this new history “makes problematic the kind of history that has been the traditional concern of the historian—political history,” and thus it “makes meaningless those aspects of the past which serious and influential contemporaries thought most meaningful” while denying that “man is . . . a rational animal, which is to say, a political animal.”³⁹

Many historians have commented on the unrecognizable picture here of both the current practice of social history and of politics itself.⁴⁰ But the embedded political value judgments in Himmelfarb’s assertions of the dignity and rationality of past politics or the privileging of “serious and influential contemporaries” and questions already “asked” by the anthropomorphised “past” may prove more consequential, for it is in the political realm that such perspectives have carried the most weight.⁴¹

This has recently been amply demonstrated in two controversies: the punishing campaigns against the Enola Gay exhibit at the Smithsonian Institution’s Air and Space Museum and the proposed *National Standards for United States History*. Both have profound implications for the current state of the history profession in general and political history in particular. The orthodox “un-revisionist” vision of political history animating these campaigns might seem to offer an opportunity for political historians. Instead, it provides a challenge to the entire profession—the response to which, ironically, affords political history a vital role. This

³⁸ Eric Foner, “The Meaning of Freedom in the Age of Emancipation,” *Journal of American History*, 81 (September 1994): 443; Lloyd Kramer, Donald Reid, and William L. Barney, eds., *Learning History in America: Schools, Cultures, and Politics* (Minneapolis, Minn., 1994), 5.

³⁹ Gertrude Himmelfarb, *The New History and the Old* (Cambridge, Mass., 1987), 14–15, 24, 22, 18, 26.

⁴⁰ See, for example, Joan W. Scott, review of Gertrude Himmelfarb, *The New History and the Old*, in *AHR*, 94 (June 1989): 699–700.

⁴¹ Political historian Alan Brinkley instead points out that “most good history . . . attempts to perceive in the past a set of patterns and connections, a coherence, even a ‘structure,’ that were *not* visible to contemporaries.” He goes on to reject the notions that “it is the task of the historian to ennoble humanity, to reveal mankind at its most ‘elevated’ and ‘dignified,’” and that political actors have a corner on humanity’s “most ennobling achievements.” Alan Brinkley, “Comments on Gertrude Himmelfarb, ‘The New History and the Old,’” May 13, 1989, 5, 10 (ms. in author’s possession).

challenge indicates how far the profession has to go in enhancing a critical public understanding of history and how vital it is that political historians in particular move beyond the complexities of modern historical practice, not by disregarding or dismissing them (as critics counsel) but by incorporating and restructuring them.

ANY HISTORIAN who has ever participated in a historical anniversary commemoration could have predicted trouble for an exhibit that combined survivors' celebrations over the end of a world war with historians' analysis of the impact and legacy of the most destructive weapon in human history. Participants naturally have an interest invested in their often hallowed memories of what many recall as their closest brush with greatness. To crash such parties with interpretations that challenge those memories, recontextualize them, or offer anything other than the public validation and heroic narrative that participants feel they have earned, seems no less heartless than the urge to use a eulogy as an opportunity to "set the record straight."

Yet these commemorations are not for participants alone; they provide broader publics a time to draw collective meanings from the event. And here, in helping to draw on and shape public memory, historians have an inescapable role—not so much because they are opportunists in search of ready audiences but because it is their job.

Historical museums face such choices every day; public history does, after all, demand consideration of public sensitivities and expectations. But as historians with their emphasis on "interpretive skepticism" play ever more important roles in exhibitions, those museums are becoming, in Edward Linenthal's words, "'forums' encouraging discussion and analysis . . . rather than . . . 'temples' of enshrined artifacts"—where conventional wisdom can be questioned rather than nostalgically celebrated through "curiosity in a case."⁴² That transition can leave them on the front lines facing the political assault on new historical interpretations, perhaps the clearest case in which historians have come to grief because of their enlarged audience.

It is that wider debate over the legitimacy of alternative historical interpretation itself, rather than the vexing tension between what Linenthal calls "the commemorative voice and the historical voice" in assessing the atom bomb's rationale and impact, that represents the most disturbing aspect of the Enola Gay controversy.⁴³ Veterans' groups went beyond the view of the Enola Gay as the air force's moral equivalent of a sacred battlefield; they also demanded exclusion of certain memoranda and other evidence calling into question the comforting claim that the atomic bombing ultimately saved lives. The planned photographs of Hiro-

⁴² Eric Gable and Richard Handler, "The Authority of Documents at Some American History Museums," *Journal of American History*, 81 (June 1994): 120; Edward T. Linenthal, "Can Museums Achieve a Balance between Memory and History?" *Chronicle of Higher Education*, 41 (February 10, 1995): B2; "A Museum in Crisis," *U.S. News & World Report* (February 13, 1995): 74. The integration of museum research into the "scholarly mainstream" is perceptively discussed in John D. Krugler, "Behind the Public Presentations: Research and Scholarship at Living History Museums of Early America," *William and Mary Quarterly*, 48 (July 1991): 352–54, 385.

⁴³ Linenthal, "Can Museums Achieve a Balance," B1.

shima victims encountered similar objections. Though an essential part of any but the most univocal and antiseptic history of the bomb dropped by the Enola Gay, such reminders of the war's costs would have cast a pall over any celebratory commemoration.

Even the many historians who deplored the ramifications of declaring contested historical findings or subjects "too hot to handle" thus cannot have been entirely surprised at the ultimate decision to jettison the exhibit's 600-page script and to favor the commemorative over the critical historical voice by presenting the plane's fuselage in what Smithsonian representatives now label a minimalist display rather than an interpretive exhibition.⁴⁴ But the logic behind the decision moved well beyond solicitude for the memories of veterans. Congressional pressure was intense, including a unanimous Senate resolution terming the exhibit "revisionist, unbalanced, and offensive" and unsubtle threats to slash both the Smithsonian's budget and the throat of its Air and Space Museum director. (The budget has indeed been cut and the director forced from office.) One new congressional member of the Smithsonian's Board of Regents described the exhibit as "historically inaccurate and not in line with the thinking of most Americans," adding, "We've got to get patriotism back into the Smithsonian. We want the Smithsonian to reflect real America and not something that a historian dreamed up."⁴⁵ Speaker of the House Newt Gingrich portrayed the Smithsonian's capitulation as "a reassertion and a renewal of American civilization." Another congressman warned that the exhibit had gotten "into areas of revisionist history and political correctness."⁴⁶ Indeed, "revisionist" history was the prime villain of the piece; as Alfred Young has warned, "the underlying assumptions of the public criticism of many exhibits is a rejection of almost two generations of historical scholarship."⁴⁷

Moreover, the terms of capitulation by what is, after all, the nation's preeminent historical museum had serious implications for the practice of history. Not only did the Smithsonian's secretary, I. Michael Heyman, imply that the academic influence on the Air and Space Museum had been too great, he also raised the question of whether it might be better "to focus the interpretation and context offered within an exhibit on explaining events as they were understood and experienced by the people living at that time," leaving "the questions that have come up since" to books and symposiums.⁴⁸ Especially because much of the objectionable evidence was in fact drawn from "people living [or dying] at that

⁴⁴ "Enola Gay Controversy Continues," *OAH Newsletter*, 23 (February 1995): 3. The intermediate decisions simply to alter the historical narrative and remove offending pictures to assuage the critics—what a group of leading scholars on the subject branded a "historical cleansing"—are of course another matter.

⁴⁵ Alfred F. Young, "S.O.S.: Storm Warning for American Museums," *OAH Newsletter*, 22 (November 1994): 6; "Enola Gay Controversy Continues," 3; "Museum in Crisis," 74; *St. Louis Post-Dispatch*, May 3, 1995, 3A.

⁴⁶ "Enola Gay Controversy Continues," 3.

⁴⁷ Young, "S.O.S.," 6; Linenthal, "Can Museums Achieve a Balance," B1.

⁴⁸ "Museum in Crisis," 75. Heyman later clarified his intentions by simply annulling historical debate in favor of sanctified interpretation. Thus he declared that the new decontaminated script would report just "the facts"—"something along the lines" that the atomic bombing of Hiroshima "resulted in saving untold numbers of lives by avoiding the necessity of invasion." *Washington Times* (March 11, 1995): A5.

time," the implication was clear: it was the contemporary *official* story that could not be challenged, at least insofar as it had become part of conventional wisdom. The museum community as a whole could not miss the chilling message, one that has only been confirmed by the Smithsonian's continuing retreat in ensuing months.

This displacement of a historical story by an official one, of a critical voice by a commemorative one, raised difficult issues in the Enola Gay controversy. But when this privileging of a celebratory voice moved beyond commemorations to the teaching of history itself, as in the campaign against the *National Standards for United States History for Grades 5–12*, the implications for the history profession were even more profound. The National Endowment for the Humanities (NEH) and the Department of Education funded this project beginning in 1992, as part of a broader effort to establish voluntary disciplinary guidelines to help local school districts reform their curricula and improve student performance. These guidelines, which included both the standards themselves and various creative examples of classroom projects that could enliven and apply them, were produced through a collaboration of an exceptional range of historical and other professional organizations, including the AHA, the OAH, the National Council for History Education, and the Organization of History Teachers.⁴⁹

Even before this document was officially released in late 1994, Lynne Cheney, who as chair of the NEH in the Bush administration had originally approved the project, began her public attacks on it for "political correctness" and its slighting of "traditional history." Radio host Rush Limbaugh followed by declaring this "insidious document" (apparently sight unseen) a "politically correct version of American history" that should be "flushed down the toilet."⁵⁰ It was downhill from there. The low point came with a Senate vote on a resolution denouncing the proposed voluntary standards, urging that they not be certified and pointedly stating that any future recipient of federal funds to develop standards "should have a decent respect for the contributions of western civilization, and United States history, ideas, and institutions, to the increase of freedom and prosperity around the world."⁵¹ This Senate resolution passed 99 to 1, with the one holdout feeling that the resolution had let the standards off too lightly.⁵²

This was admittedly a minor, unexpected, toothless "sense of the Senate" resolution. Some who went along with it did so as a tactical compromise because it would have no legal force, unlike the proposed amendment it replaced, and because the authors of the condemned standards had already committed themselves to revising them. Others supported it as a slap at the NEH and at what appeared to the new Republican Congress to be an overly intrusive Washington effort to promote common educational standards. Still, there seems no way around the conclusion that the vote represented a stinging rebuke not just to the standards but to the history profession that produced them as well.

⁴⁹ National Center for History in the Schools, *National Standards for United States History: Exploring the American Experience, Grades 5–12* (Los Angeles, Calif., 1994).

⁵⁰ Gary B. Nash, "National Standards in U.S. History: A Note from the President," *OAH Newsletter*, 22 (November 1994): 1, 16.

⁵¹ *Congressional Record*, 104th Cong., 1st sess. (January 18, 1995): S1039.

⁵² John Leo, "History Standards Are Bunk," *U.S. News & World Report* (February 6, 1995): 23.

The debate itself seems to indicate that. The Senate discussion, heavily and repetitively reliant on material provided by Lynne Cheney (now of the American Enterprise Institute) and her Committee to Review National Standards (which includes historians such as Gertrude Himmelfarb), was labeled by NEH chair and historian Sheldon Hackney a “drive-by debate” that held school reform hostage to “the culture wars.” The standards were characterized as “grotesquely biased,” “insulting,” “perverse and distorted,” and “hostile to the main threads of American history”—all in all, a “travesty” that ignored “the most important achievements of our history.” They were deemed so full of “the kind of valueless, all-points-of-view-are-equally-valid nonsense” that they might “put our children at risk of not being fairly and broadly educated.”⁵³

What accounts for the overwrought tone of such attacks? Historians reading the standards document, even while differing over which perspectives and topics deserve greatest attention, are most likely to be struck by its success at accommodating and allowing for differing pedagogical and interpretive approaches. The principal specific charges against it also crumble at the touch. One example is the misleading claim that “the Constitution is not mentioned in the 31 core standards”; in fact, one standard covers “the foundation of the American political system” and then includes separate sections on the Constitution and the Bill of Rights. Issues of constitutional development emerge repeatedly in the document.⁵⁴ Other charges are directed not against the standards themselves but against a deceptive sample from the hundreds of illustrative lessons—which were designed in part, ironically, to *broaden* consensus by recognizing the constructive relationships that have developed between historical work in colleges and secondary schools and by showcasing the role of practicing teachers in the document.

Particularly for the purposes of this article, the merits of this critique are almost beside the point. What is relevant is the critics’ view of the historical profession and its abandonment of what they see as history’s, especially political history’s, proper role. This challenge goes to the very heart of the historical enterprise, for the underlying assumption seems to be that history is no place for non-“traditional” historians, that “the American people would be foolish” to let the view of these “politically correct” hijackers of history “anywhere near their schools.”⁵⁵

If this battle were simply a political one, between left-wing professorial propagandists of “political correctness” and right-wing advocates of “patriotic correctness,” the current controversy might be easier to understand. And there are unmistakable political influences in the way that conservative individuals and groups such as Lynne Cheney, William Bennett, the Family Research Council, and the *Wall Street Journal* have tied this issue to the Republican election victories in 1994 and have used it to activate fundamental suspicions of alien “cultural elites,” who, after all, do make convenient scapegoats amid a rapidly changing, interde-

⁵³ “An Update on National History Standards,” *OAH Newsletter*, 23 (February 1995): 3; *Congressional Record* (January 18, 1995): S1039, S1033, S1034, S1035, S1032.

⁵⁴ National Center for History in the Schools, *National Standards for United States History*, 82, 84, 86; Robert C. Johnston and Karen Diegmüller, “Senate Approves Resolution Denouncing History Standards,” *Education Week*, 14 (January 25, 1995): 14.

⁵⁵ John Leo, in *Congressional Record* (January 18, 1995): S1035.

pendent world economy and communications network that erodes and destabilizes local community values while widening the gap between ordinary Americans and more privileged, educated ones.⁵⁶ I was struck by one Pennsylvania congressman's lament that the standards revealed the impact of "too many California historians."⁵⁷

But, as the 99–1 vote itself may suggest, as the often unsympathetic media coverage of the proposed standards may confirm, and as the extravagant distortion of the document's actual content underscores, what is being attacked goes beyond the surface of contemporary politics, beyond the chimera that students are emerging from history classes or visitors from museum exhibits as wild-eyed radicals. What is at issue is a vision of history and of America that transcends partisan politics. "The history that many of us who are older learned in school" provides a benchmark, in that it highlighted "core information about who we are as a nation and how our world and our Nation have progressed over time."⁵⁸ This conception of history places a premium on inculcating an affirmative patriotism, one that might be contrasted, for example, with the outlook of late nineteenth-century historians, who considered it essential to combine nationalist values with active problem-solving and interpretive skills in training the nation's future leaders. Virtually nowhere in the assault on the history standards did critics acknowledge the assumption that many educators today take for granted: a national identity based on airbrushed history is neither secure nor consistent with a faith in informed democratic processes to bring constructive change.⁵⁹ Instead, critics prescribed an image-building task for historians: forging a sense of self-worth and self-definition among their students based on a combined pride in past American leaders, American ideas of economic and political freedom, and "the positive and optimistic accomplishments and nature of the American people."⁶⁰

As a foundation, this historically constructed national identity relies on a memorizable list of heroes and leaders. To the extent that this list and the system it celebrates constitute the core of American history, with critical thought and interpretation as afterthoughts at best, immense importance attaches to any omitted name, particularly those seen as contributing to the triumphs and progress of American politics or individualist free enterprise. Critics, for example, commonly castigated the report for failing to mention Thomas Edison by name, even though the brief draft of standards, which focused on overarching changes and themes and made no pretense of replacing a textbook, included various components and questions relating inventions and technology to economic development and everyday life.⁶¹ Another frequent complaint was the standards' excessive attention to the worst aspects of American history, such as McCarthyism

⁵⁶ See, for example, "The Trend of History," *Wall Street Journal* (January 31, 1995): A20.

⁵⁷ "National History Standards Could Sink Goals 2000, Rep. Goodling Warns," *Education Week*, 14 (February 1, 1995): 18.

⁵⁸ *Congressional Record* (January 18, 1995): S1032.

⁵⁹ See Douglas Greenberg, "Face the Nation," *Chicago Tribune* (January 9, 1995): 11.

⁶⁰ *Congressional Record* (January 18, 1995): S1033.

⁶¹ National Center for History in the Schools, *National Standards for United States History*, 138, 181.

(commonly equated with Joseph McCarthy himself, whose impact, one Senator explained, had been "passing at best").⁶²

This approach toward precisely what should be studied combines with strong assumptions about whose history is worthy of attention, or is worthy at all. Especially revealing is the juxtaposition of claims that the report places American history and achievements in an unrelentingly negative light while it at the same time romanticizes the accomplishments of politically correct minorities. This implicit answer to Crèvecoeur's classic question, "What then is the American, this new man?" is confirmed by concerns that the standards exaggerate the contributions of "preliterate" African and Indian cultures as opposed to Western Civilization.⁶³

Even more damning than the belief that the proposed standards represent an overinclusive and unpatriotic version of history was the conviction that this distortion was so ingrained in "the state of the study of history in higher education" that no "national group of historians" would be able to produce acceptable standards.⁶⁴ Revealingly, the one charge that almost never appeared in the case against the history standards is that they failed to reflect contemporary scholarship; they instead reflected it all too well. In the glowering assessment of one member of the Committee to Review National Standards: "That so many academic historians, high school teachers and professional organizations created or approved such a biased document is the real story in this controversy."⁶⁵ The authors of the proposed standards were cast as "the community of historians who dominate the intellectual process of defining our history in this country"—a group with "virtually no interest . . . in factual history" but instead a dangerous commitment to "revisionist history and politicized history and editorialized history."⁶⁶

This epithet, "revisionist," which came up repeatedly, both here and in the Enola Gay controversy, may be the key to understanding the current crisis of history. "Revisionist" meant the displacement of the more happy-faced, elite-oriented view of American progress and destiny to which most Americans, particularly those raised on "consensus history" textbooks, had become accustomed.⁶⁷ At the same time, the use of "revisionist" as a term of abuse suggested a rejection of the very notion of historical reinterpretation, under the assumption that the displaced version of history had been objective and factual, while revisions were subjective and faddish.

IT IS ONLY IN THE CONTEXT of this chasm between the popular and the historical stance toward revisionism that one can make sense of the current historical crisis.

⁶² *Congressional Record* (January 18, 1995): S1038.

⁶³ Leo, "History Standards Are Bunk," 23.

⁶⁴ Lynne V. Cheney, quoted in Page Putnam Miller, "NCC Advocacy Update: National Endowment for the Humanities Faces Major Cuts or Possible Abolishment," *AHA Perspectives*, 33 (March 1995): 17.

⁶⁵ Sandra Stotsky, "Letter to the Editor," *New York Times* (February 2, 1995): A14.

⁶⁶ *Congressional Record* (January 18, 1995): S1038–39.

⁶⁷ See Gary B. Nash, "The History Children Should Study," *Chronicle of Higher Education*, 41 (April 21, 1995): A60.

Most historians see revision—from the right, the left, or the center—as vital to the profession's development, as long as that revision meets standards of logic and evidence. They draw a distinction between the legitimacy of a historian's political perspective, in which historical questions are conditioned by personal values and concerns, and the illegitimacy of politicized history, in which one version is proclaimed as superior to all others based on its political message. Just as it is impossible, most say, to show that any given interpretation is objectively true and complete, it is essential that all interpretations be open to testing by available evidence, allowing some renderings to be rejected as less successful than others at incorporating that evidence. Abandon that principle to a mandated, politically desired conclusion and something much more important to historians than any particular alternative interpretation is lost—including the only sort of history from which a democratic society can hope to learn. History and recent politics (especially in the totalitarian systems from which critics are most eager to distinguish the United States) are littered with evidence of the tragic consequences of history in the service of patriotic orthodoxy or an official story. In the classic formulation of Herbert Butterfield, "If men at twenty learn to see the events of history in a certain framework, and if they learn it without acquiring imagination and elasticity of mind—then we can say . . . that by the study of history a merely probable national disaster can be converted into a one hundred per cent certainty."⁶⁸ Historians are as susceptible to self-righteousness and self-delusion as anyone else, but they seem on solid ground in viewing this bedrock commitment to critical thought, to revising and questioning, as a professional rather than a political one.

To many critics, however, the "revisionist" interpretive process itself, not the effort to salvage orthodox interpretations from new critical perspectives, constitutes illegitimate politicization. At base, the very process of encouraging "consumers" of history to think and to challenge frameworks usually taken for granted is cast as political. For universities and, more recently, for museums, this view of politics may be most chilling because it is inescapable, for this formulation of "politics" is inherent in their mission.

There is an ironic overlay here, as critics successfully enlist public feelings that history should be neutral and static in order to serve openly political purposes. But the impact of this conception of "correct" history is no less real. Thus, when Eric Foner condemned the Senate resolution for promoting "an official interpretation of American history," and when he added that "this kind of thing used to happen regularly in other countries" but is "inappropriate for a society that values freedom of thought," he may have been preaching mainly to the converted.⁶⁹ And, despite the efforts at various levels to open up the historical interpretive process to broader audiences, the converted are still too small a group.

Historians cannot entirely escape the blame for this, nor can they simply retreat to the hope that matters will improve once the expanded audience exposed to critical history moves into positions of influence. For historians may have brought

⁶⁸ Herbert Butterfield, *Christianity and History* (New York, 1950), 15–16. I am grateful to Paul Schroeder for directing me to this quotation.

⁶⁹ Eric Foner, "Letter to the Editor," *New York Times* (January 31, 1995): A10.

part of the current crisis upon themselves—not just by failing to engage some audiences but by teaching others too well. Postwar consensus political history, or more precisely the textbooks that it spawned, helped establish the ideal that is now being used to reject recent work.

Like most labels, “consensus history” encompasses such a vast range of scholarship that it calls into question the term’s usefulness. It includes some of the most sophisticated and respected works the profession has produced. Even those such as Richard Hofstadter or Louis Hartz, who found much to condemn in “the American political tradition” they described, embraced the view that it had been characterized by a “pervasive American consensus,” a “common climate of opinion” embodying “an almost universal commitment to economic self-aggrandizement through competitive capitalism.”⁷⁰ The combination of the victory against the total evil of Hitlerism in World War II and the battle against the total evil of communism in the Cold War had the effect of entrenching American nationalism and sanctifying American virtues and myths. Capitalism, “free enterprise,” and democracy were not only inextricably linked, in some hands they were equated. Consensus historians joined social scientists of the 1940s and 1950s in minimizing the role of class conflict and depicting many resisters to the political order as unenlightened, maladjusted malcontents. At the same time, in failing to pursue the question of who lived outside the consensus, they “underplayed the scope for agency and the significance of the histories of blacks, women, workers, and immigrants.”⁷¹

Especially once the subtleties of this history were leveled by secondary school textbooks, it provided a base for later beliefs that critical perspectives and more inclusive historical treatments were illegitimate and biased. A resolution from the Texas state legislature urged that “the American history courses in the public schools emphasize in the textbooks our glowing and throbbing history of hearts and souls inspired by wonderful American principles and traditions.”⁷² As Frances FitzGerald observes in her study of history textbooks, inside the covers of 1950s texts, “America was perfect: the greatest nation in the world, and the embodiment of democracy, freedom, and technological progress.” The orthodoxy of these texts, or so it seemed to their young readers, “left no handholds for attack, no lodging for decay.”⁷³ The conventional wisdom was entrenched by what Joyce Appleby has termed “induced amnesia,” by the parts of America and American history that were left out, and it was sustained at a sacrifice of critical analysis and other professional goals that most historians today seem unwilling to relinquish.⁷⁴

This orthodoxy and the anti-historical attitudes that bolstered it now survive to haunt the history profession in the form of criticism of the Enola Gay exhibit and of the proposed history standards. But if an oversimplified incarnation of

⁷⁰ Terrence J. McDonald, “The Burdens of Urban History: The Theory of the State in Recent American Social History,” *Studies in American Political Development*, 3 (1989): 4; Alan Brinkley, “Prosperity, Depression, and War, 1920–1945,” in Foner, *New American History*, 122.

⁷¹ Ira Katznelson, “‘The Burdens of Urban History’: Comment,” *Studies in American Political Development*, 3 (1989): 32.

⁷² Stephen J. Whitfield, *The Culture of the Cold War* (Baltimore, Md., 1991), 56.

⁷³ Frances FitzGerald, *America Revised: History Schoolbooks in the Twentieth Century* (Boston, 1979), 10.

⁷⁴ Joyce Appleby, “Recovering America’s Historic Diversity: Beyond Exceptionalism,” *Journal of American History*, 79 (September 1992): 429.

consensus history now encumbers historians, perhaps that is only just, for they have profited for decades off the claimed shortcomings of the “consensus school.” If historians had not had a consensus school to vilify, they might have had to create one, as arguably they did, by depriving it of its subtlety. The textbook version of this consensus school, paralleled by beliefs of the general populace and—perhaps most of all—of “informed opinion,” could turn unremarkable evidence of dissent, power politics, or agency of non-“mainstream” groups into instant news.

In the 1960s and 1970s, the “organizational” or “modernization” thesis took hold, with its emphasis on an irreversible, all-encompassing “shift from small-scale, informal, local or regionally oriented groups to large-scale, national, formal,” more centralized, bureaucratized organizations.⁷⁵ This thesis was vulnerable; as is so often the case, historians embraced this social science model just as other fields were disgoring it. But it, like the consensus school with which it had some affinity, served as a ready straw man, lending the luster of refutation to evidence of “resistance” to the advance of acquisitive capitalism and “modernist values.”

By the late 1970s and 1980s, “republican ideology”—the emphasis on the superiority of independent “producer values” over the special privileges of employers or other authorities and of commitment to a common good over unbridled self-interest—was exhibit number one for this emphasis on a political culture of “resistance.” Extending from the eighteenth century into the twentieth, in somewhat diffuse and chameleon-like fashion, republican themes were depicted as animating everything from the American Revolution to Jacksonian workingmen’s politics to the labor struggles of the 1930s.⁷⁶ Social movements and mass movement-building, dismissed by some consensus theorists in terms of an irrational “paranoid style” of those whom modernity had left behind, now tend to be viewed as rational, *political* actions grounded in an alternative “movement culture.”⁷⁷ This applies not just to mass movements such as the Populists or the civil rights movement with which historians are decidedly sympathetic. Even such groups as the Ku Klux Klan or the New Right, which liberal historians tend to view with repugnance, are still depicted, in certain recent treatments, as rejecting what earlier historians had “come to believe are the uncontested assumptions of modern Western society,” as embodying certain rational grievances against elites, and as “appealing to the normative beliefs of ordinary, native-born white Protestants.”⁷⁸ The framework of mainstream modernizing “consensus” values

⁷⁵ Louis Galambos, quoted in Brinkley, “Prosperity, Depression, and War,” 121.

⁷⁶ See Daniel T. Rodgers, “Republicanism: The Career of a Concept,” *Journal of American History*, 79 (June 1992): 11–38; Gary Gerstle, *Working-Class Americanism: The Politics of Labor in a Textile City, 1914–1960* (Cambridge, 1989).

⁷⁷ Contrast Richard Hofstadter, *The Paranoid Style in American Politics and Other Essays* (New York, 1965), with: Lawrence Goodwyn, *The Populist Moment: A Short History of the Agrarian Revolt in America* (New York, 1978); Robert C. McMath, Jr., *American Populism: A Social History, 1877–1898* (New York, 1993); Brinkley, “Prosperity, Depression, and War,” 125; Robert A. Goldberg, *Grassroots Resistance: Social Movements in Twentieth Century America* (Belmont, Calif., 1991).

⁷⁸ Alan Brinkley, “The Problem of American Conservatism,” *AHR*, 99 (April 1994): 423; Michael Kazin, “The Grass-Roots Right: New Histories of U.S. Conservatism in the Twentieth Century,” *AHR*, 97 (February 1992): 140. Also see Shawn Lay, “Hooded Populism: New Assessments of the Ku Klux Klan of the 1920s,” *Reviews in American History*, 22 (December 1994): 668–73.

versus marginal challengers has given way to one in which those very values were contested in the political sphere by groups who were anything but marginal.

Consensus political history had also taken for granted a wide enough area of social concord, especially by the twentieth century, that any notion of elite political or social domination seemed harsh, unsophisticated, and wrong. The multiple sources of power in American society assured that no individual group could dominate; government instead arrived at policy through a pluralistic, mediating process of "ad hoc, cross-class, group political alliances rather than class alliances."⁷⁹ This view, too, proved an inviting target, especially to political radicals of the 1960s, dubiously lumped together as the New Left, who assailed the "consensus liberalism" of Presidents John F. Kennedy and Lyndon B. Johnson as a veil for, if not the vehicle of, malign corporate power in America. Even such celebrated liberal reforms as the Progressive movement and the New Deal, they argued, were actually instances of "corporate liberalism"—initiated by the corporate power structure or those it influenced to quell potentially populist or socialist discontent and to promote stabilized markets and a smoother, less risky path to economic growth.⁸⁰ In its most extreme form, this interpretation foundered; "corporate liberal" businessmen, it turned out, usually had less political clout than their more numerous and more conservative colleagues; moreover, other groups and forces usually could be shown to have greater influence, even when supporting legislation that "corporate liberals" also favored.⁸¹

Yet the corporate liberal theorists helped redirect historical attention both toward the failures of a pluralist model to reflect the disparities of power in American society and toward the failures of modern liberalism to rectify those disparities. Political historians of the twentieth century now tend to draw on theories of state power that suggest patterns of political influence more complex than corporate liberalism. They point to some areas in which organized interests do have greater influence, other areas in which policy makers are inclined to act on more independent "regime imperatives," and many areas in which policy is constrained by the need to accommodate the capitalist economic powers without which a politically acceptable economic performance would be unobtainable.⁸² The New Left condemnation of certain liberal programs for disserving the disempowered, however, has retained influence. In the current climate of shrunken reform possibilities, program failures can be persuasively explained through

⁷⁹ McDonald, "Burdens of Urban History," 10.

⁸⁰ The usual suspects are Gabriel Kolko, *The Triumph of Conservatism: A Re-interpretation of American History, 1900–1916* (New York, 1963); James Weinstein, *The Corporate Ideal in the Liberal State, 1900–1918* (Boston, 1968); and Barton J. Bernstein, "The New Deal: The Conservative Achievements of Liberal Reform," in Bernstein, ed., *Towards a New Past: Dissenting Essays in American History* (New York, 1968), 263–88.

⁸¹ See Theda Skocpol, *Protecting Soldiers and Mothers: The Political Origins of Social Policy in the United States* (Cambridge, Mass., 1992), 27–30; Ellis W. Hawley, "The Discovery and Study of a 'Corporate Liberalism,'" *Business History Review*, 52 (Autumn 1978): 309–20.

⁸² Michael J. Hogan, "Revival and Reform: America's Twentieth-Century Search for a New Economic Order Abroad," *Diplomatic History*, 8 (Fall 1984): 287–310; John Braeman, "The New Deal: The Collapse of the Liberal Consensus," *Canadian Review of American Studies*, 20 (Summer 1989): 77; Donald T. Critchlow, "Introduction: Social-Policy History: Past and Present," in Critchlow and Ellis W. Hawley, eds., *Federal Social Policy: The Historical Dimension* (University Park, Pa., 1988); Larry G. Gerber, "A Reply to William Becker," *Journal of Policy History*, 5 (1993): 370; Ellis W. Hawley, "Social Policy and the Liberal State in Twentieth-Century America," in Critchlow and Hawley, 124.

conservative constraints beyond liberals' control.⁸³ But other recent historians have continued to acknowledge liberals' "missed opportunities, unintended consequences, and dangerous but inescapable compromises." They highlight the failures of politicized unions and the fifty-year reign of a "New Deal order" to provide a sustainable social democratic coalition and to prevent the subordination of workers' class and communal interests.⁸⁴

IN INTERROGATING MODELS of political modernization, pluralist politics, and modern liberalism, American historians have been true to their origins, pursuing questions that reveal a continuing conversation between history and political science. Links have been even more evident in two other joint efforts: the "new political history" to convert historians to political scientists' reliance on quantitative methods and rigorous "scientific" testing of hypotheses, and the "new institutionalism" of political science, which brought historical issues of public policy development into a new position of prominence.

The story of the "new political historians" so lends itself to caricature that one is tempted to leave it at that. They began in the 1960s and 1970s with a brash, scientific self-confidence befitting an advance guard, with new journals (*Social Science History*, the *Historical Methods Newsletter*, the *Journal of Interdisciplinary History*), new data clearinghouses and summer institutes, new history department positions to teach new courses in computing and quantitative methods, and the lion's share of cutting-edge grant money.⁸⁵ By 1970, noted J. Morgan Kousser, "quantitative social science" had triumphed in its "invasion of American history." The "noisy initial skirmishes were over. Formidable beachheads of research had been established in social and political history."⁸⁶ Increased legitimacy and renown followed in the 1970s, prompting Kousser to adopt the perspective of the colonial victor over his backward colleagues who had not yet assimilated to the superior culture of quantification: "Can the average citizens of the increasingly colonized country afford to remain semiliterate in the traders' language?"⁸⁷ Other social science historians directed particular attention to their "ethnocultural" interpretation of voting, which asserted on the basis of elaborate statistical analysis of nineteenth-century elections that "partisan affiliations were not rooted in economic class distinctions. They were political expressions of shared values derived from the voter's membership in and commitment to ethnic and religious groups."⁸⁸

By the 1980s, however, the new political history was on a path from quantifica-

⁸³ Anthony J. Badger, *The New Deal: The Depression Years, 1933–1940* (New York, 1988).

⁸⁴ Steve Fraser and Gary Gerstle, eds., *The Rise and Fall of the New Deal Order, 1930–1980* (Princeton, N.J., 1989); Alan Brinkley, *The End of Reform: New Deal Liberalism in Recession and War* (New York, 1995).

⁸⁵ Allan G. Bogue, "The Quest for Numeracy: Data and Methods in American Political History," *Journal of Interdisciplinary History*, 21 (Summer 1990): 98–103.

⁸⁶ J. Morgan Kousser, "Quantitative Social-Scientific History," in Michael Kammen, ed., *The Past before Us: Contemporary Historical Writing in the United States* (Ithaca, N.Y., 1980), 433, 435–36.

⁸⁷ Kousser, "Quantitative Social-Scientific History," 456.

⁸⁸ Paul Kleppner, 1970, quoted in Allan G. Bogue, "The New Political History in the 1970s," in Kammen, *Past before Us*, 241.

tion to qualification. In 1994, some fell back on the entreaty that their work had been misrepresented, miscited, and misconstrued.⁸⁹

Clearly, what became almost a cottage industry of criticism of the new political history had taken its toll.⁹⁰ But, while the days of historical treatments limited to quantifiable hypotheses (however trivial) and driven by tabular presentations (however tedious) seem to be behind us, the “ethnocultural” focus on voters underscored the need for more direct inquiry into the political influences on and world views of ordinary people. And even one of the less developed connections in the new political history—how politicians, in the absence of direct mandates from the public, decide what initiatives to pursue—impelled historians to direct their attention to policy formation itself.

Here, another link with political science has played a crucial role. Political scientists shared the frustration of political historians with what they saw as the narrow band of “quantifiable,” testable questions centering on statistical studies of individual or legislative voting patterns. A different approach was needed to answer questions about the development of the American welfare state in particular and the government’s role in general. They embraced a “new institutionalism” (so named because it was something of a return to the “old” institutionalist approach of the history and political science professions’ early years), establishing the journal *Studies in American Political Development* in 1986 and forming in 1990 a History and Politics section—which today has over 500 members—of the American Political Science Association.⁹¹

Governmental institutions and processes, the new institutionalists argue, cannot be reduced to constituent political preferences. State officials have considerable leeway in setting political agendas. Just as important, the structure of the polity—its party system, its “state capacity” (the constraints of mobilizable expertise, finances, and ideology that limit the tasks a central bureaucracy can handle), the legacy of its past policies, the access points it establishes for outside groups—can be a much better guide to matters attracting (or failing to attract) political attention than vague theories of consensus “national values” or broad structures of social power.⁹² This attention to the specific structural context in which institutions work has given new life to the history of public policy, including the establishment of the interdisciplinary *Journal of Policy History* in 1989, leading one exponent to muse that such interdisciplinary “history has the potential to become a unifying force in social science once again.”⁹³ Thus political scientist Stephen Skowronek, in a book that first introduced many political historians to the new institutionalism, argued that the U.S. political system in the late nineteenth century was largely a “state of courts and parties,” in which the focus of political parties on their control of patronage jobs and the rules imposed by the courts on

⁸⁹ Ronald P. Formisano, “The Invention of the Ethnocultural Interpretation,” *AHR*, 99 (April 1994): 456–57, 469.

⁹⁰ The sources cited above provide citations to a large number of these critiques. Two particularly succinct critiques are Daniel Feller, “Letter to the Editor,” *AHR*, 99 (December 1994): 1822–24; Sean Wilentz, “On Class and Politics in Jacksonian America,” *Reviews in American History*, 10 (December 1982): 48–50.

⁹¹ Robertson, “History, Behavioralism, and the Return,” 134–35.

⁹² See Skocpol, *Protecting Soldiers and Mothers*, 41.

⁹³ Robertson, “History, Behavioralism, and the Return,” 140.

state/society/business relations heavily influenced the activities of an otherwise weak and localized government sector, while the growth of a more centralized, nonpartisan civil service in the twentieth century followed to some degree from the efforts of administrators themselves to escape from the control of the courts and an enfeebled party system.⁹⁴

This dense, internal analysis of state imperatives, if adopted as a complete model rather than a supplementary tool, might have isolated political historians. But, rather than imprisoning themselves within the very institutions from which others had fled, recent political historians have adapted the work of gender and social historians.

For most historians, especially political historians, to give more than a second thought to how sex roles were constructed cut against the professional grain. The profession tended to marginalize both women historians and history involving women as signs “of the amateur and of bad history.”⁹⁵ Certain developments in women’s studies in the late 1960s and 1970s, especially the literature on the evolution of a separate, private, “women’s sphere,” only encouraged some political historians in the view that considerations of gender were someone else’s problem, since politics—with the main exception of the women’s suffrage campaign—was in the male sphere.

In 1984, however, a largely synthetic article by Paula Baker in the *AHR* practically forced political historians to confront the wide-reaching implications of gender analysis for their field. The very fact that nineteenth-century partisan politics was deemed a male preserve, she pointed out, revealed a great deal, both about politics and about male identity. Baker went on to review the established evidence of women’s political culture, which took middle and upper-class women beyond Republican Motherhood to apply their domestic ideals outside the home—through voluntary associational actions that targeted such community evils as slavery, urban poverty, lynching, ill health, illiteracy, and moral decay (particularly alcohol abuse).⁹⁶ These activist efforts proved to be critical, she argued, not only to the shifting balance between political parties and political pressure groups but also to a transformation of the political agenda, “a domestication of politics,” that represents the decisive transition in the development of the American welfare state. As William Chafe observes, so much of what “we find admirable about the Progressive Era appears to have been associated in one way or another with women reformers carrying out the politics associated in the nineteenth century with women’s domestic concerns,” from factory safety and

⁹⁴ Stephen Skowronek, *Building a New American State: The Expansion of National Administrative Capacities, 1877–1920* (Cambridge, 1982).

⁹⁵ Bonnie G. Smith, “Gender and Historical Understanding,” in Kramer, Reid, and Barney, *Learning History in America*, 113.

⁹⁶ Paula Baker, “The Domestication of Politics: Women and American Political Society, 1780–1920,” *AHR*, 89 (June 1984): 620–47, reprinted in Linda Gordon, ed., *Women, the State, and Welfare* (Madison, Wis., 1990), 55–91. William H. Chafe, “Women’s History and Political History: Some Thoughts on Progressivism and the New Deal,” in Hewitt and Leacock, *Visible Women*, 103. The vitality of women’s public culture in its interactions with the male-dominated political world of the nineteenth century is systematically explored in Kathryn Kish Sklar, *Florence Kelley and the Nation’s Work: The Rise of Women’s Political Culture* (New Haven, Conn., 1995); and Mary P. Ryan, “Gender and Public Access: Women’s Politics in Nineteenth Century America,” in Craig Calhoun, ed., *Habermas and the Public Sphere* (Cambridge, Mass., 1992), 264–86.

child labor law enforcement to prison reform and women's suffrage—what is now termed a “maternalist,” nurturant political agenda.⁹⁷ The attention directed toward voluntary associations, long recognized as important in the American context, has also allowed enhanced understanding of how private reform efforts fed directly into government social welfare organizations, to create what Robyn Muncy calls “a female dominion” in which a network of women activists, both inside and outside government agencies, implemented and shaped social welfare policy.⁹⁸

The leading scholar in the “new institutionalist” campaign to “bring the state back in” to analyses of politics, Theda Skocpol, combines this expanded, gendered understanding of welfare-state development with her earlier emphasis on state structure to construct the fullest treatment to date of “the political origins of social policy in the United States.”⁹⁹ The maternalist welfare state, she argues, moved into the gap left by non-programmatic political parties, a weak bureaucracy, a labor movement divided over social legislation, and a court system hostile to the kind of workingman's welfare state enacted in Europe. Success came through a nationwide network of local women's clubs that could sway public opinion, lobby legislators, or work with the female network within governmental social-service agencies to develop, publicize, and implement reform programs.

The role of women in developing this welfare state by no means assured its benevolence; Linda Gordon and others have argued, with devastating effectiveness, that the domestic ideologies of women reformers—along with gendered and racial assumptions about, for example, work and family structure—helped lead to a multi-tiered welfare system in which programs for women and children, such as Aid to Families with Dependent Children, were degraded and underfunded compared to non-means tested programs for workers and their families.¹⁰⁰ In recognizing how public policy is shaped by the interactions between governmental structure, legislative actions, private groups, and underlying social assumptions, Skocpol and Gordon provided political historians with more sophisticated and far-reaching answers to their traditional questions on how government and its programs developed. Political historians had met the enemy: feminists, social scientists, and social historians. And, fortunately, some ultimately admitted, she is us.

This convergence of social and political history faced other barriers. Both sides viewed the other with distaste bordering on suspicion. “New social historians,” after all, had initially defined themselves in terms of their flight from a shallow history of political elites, and the prospect of becoming “a mere branch of social

⁹⁷ Chafe, “Women's History and Political History,” 105. See in particular the articles by Kathryn Kish Sklar, Seth Koven and Sonya Michel, Eileen Boris, and Molly Ladd-Taylor in Seth Koven and Sonya Michel, eds., *Mothers of a New World: Maternalist Politics and the Origins of Welfare States* (New York, 1993).

⁹⁸ Robyn Muncy, *Creating a Female Dominion in American Reform, 1890–1930* (New York, 1991).

⁹⁹ Skocpol, *Protecting Soldiers and Mothers*. Also see Edward D. Berkowitz, “How to Think about the Welfare State,” *Labor History*, 32 (Fall 1991): 492–95.

¹⁰⁰ Linda Gordon, *Pitied But Not Entitled: Single Mothers and the History of Welfare, 1890–1935* (New York, 1994), 38.

history” was enough to tempt political historians to defend narrative reportage of elections and high policy.¹⁰¹

Yet both sides came to realize that they ignored the other at their peril. “Everyone who comments on social history’s fragmentation,” notes Alice Kessler-Harris, “agrees that it somehow lost its path as a result of its disconnection from politics.”¹⁰² But more was at stake than the incoherence of the discipline, or even the mutual insights that each field could provide the other. Interaction was the only way to interrogate power—how it was structured and changed, where it was contested, how it was exerted, what its impact was, and what assumptions shaped the discourse that framed it.

Thus Richard Oestreicher successfully bridged the troubling gap in historical treatments of the period 1870–1940 between workers (motivated, according to labor historians, by republican ideology and growing class consciousness in the workplace) and working-class voters (activated, according to new political historians, through traditional, ethnoreligious loyalties). By drawing on the “new institutionalist” understanding that party systems and other political structures provided outlets for certain worker concerns while leaving others unarticulated and “politically latent,” Oestreicher showed that political and labor historians’ portrayals were not necessarily inconsistent at all.¹⁰³ Lizabeth Cohen expanded on this insight in her study of how Chicago workers between 1919 and 1939 reshaped identities and shifted loyalties once expectations of security through ethnic institutions and employers’ welfare capitalist initiatives broke down in the Great Depression.¹⁰⁴ Through new voting patterns, pressures, and protests, Cohen argues, industrial workers “made” a New Deal, characterized by an enhanced sense of political involvement and entitlement centered on a New Deal welfare state and a nascent industrial labor movement.

If something as basic to modern U.S. political history as the New Deal coalition can only be fully understood by incorporating social history, it should not be surprising that the amalgamation of political and social history is already well advanced in other areas. Alan Dawley’s synthetic account of the 1890s through the 1930s takes as its major task the integration of “multicultural complexity” and “new discoveries in social history” with “the traditional topics of politics and diplomacy.” In the process, he makes a provocative case that mass social movements and other “pressure from below . . . forced decisions upon elites that they would have preferred to avoid” and thus influenced the evolution of the American welfare state.¹⁰⁵

Race also now plays a pivotal role in political history—and not only as an issue that white politicians occasionally chose to confront. As historians from Edmund Morgan to David Roediger have shown, whites’ notions of political rights and

¹⁰¹ Paul F. Bourke and Donald A. DeBats, “Comment and Controversy I: Restoring Politics to Political History,” *Journal of Interdisciplinary History*, 15 (Winter 1985): 459–66.

¹⁰² Alice Kessler-Harris, “Social History,” in Foner, *New American History*, 178.

¹⁰³ Richard Oestreicher, “Urban Working-Class Political Behavior and Theories of American Electoral Politics, 1870–1940,” *Journal of American History*, 74 (March 1988): 1282.

¹⁰⁴ Lizabeth Cohen, *Making a New Deal: Industrial Workers in Chicago, 1919–1939* (Cambridge, 1990).

¹⁰⁵ Alan Dawley, *Struggles for Justice: Social Responsibility and the Liberal State* (Cambridge, Mass., 1991), vii–viii.

equality, far from contradicting the racial hierarchy, were actually premised on it.¹⁰⁶ Eric Foner, whose recent writing reveals his concern with transcending the “compartmentalization of historical study into ‘social’ and ‘political’ components,” uses race as the organizing principle of his writings on the Civil War and Reconstruction. “The centrality of the black experience,” he contends, is demonstrated by the fact that African Americans’ actions in the Civil War “helped force the nation down the road to emancipation,” while “their quest for individual and community autonomy” also made them “active agents in the making of Reconstruction” and in the establishment of its “political and economic agenda.”¹⁰⁷ Ideologies of race have also been shown to play a primary role in changing political allegiances since the 1960s.¹⁰⁸ The very process of defining political history has been advanced by such perspectives; Robin D. G. Kelley has directed attention to the “infrapolitics of oppressed groups,” which allows even those excluded from formal political institutions to influence them through acts of individual or community resistance that can force political change or provide a base for more organized, more overtly political, opposition.¹⁰⁹

A retreat to the nostalgia of “induced amnesia,” away from the very perspectives that have enriched our understanding of politics and political history, may seem a logical response to the challenges the profession faces, one that would surely hearten certain political actors who style themselves as friends of political history. In this retreat, after all, political historians have the tempting opportunity to take the lead. But they have other prospects. The criticisms in the Enola Gay and National Standards debates of non-“traditional” history represent an assault on perspectives that are a vital part of mainstream historiography, of political history itself, and of the common professional commitment to a process of critical analysis and reinterpretation. These attacks thus should not be taken lightly. If historians mean what they say when they talk of reducing historical fragmentation, they must build on recent trends in political history that ask new questions and encompass new groups to reveal new dimensions of public policy. If historians take seriously their civic democratic commitments to enlightened public debate and decision making that embrace the broad citizenry, they similarly can look to recent developments in political history scholarship as one model for furthering their goals. Neither professional nor civic commitments would be well served by retreating to critics’ narrower vision of the historical enterprise.

¹⁰⁶ Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975); David R. Roediger, *The Wages of Whiteness: Race and the Making of the American Working Class* (London, 1991).

¹⁰⁷ Eric Foner, *Reconstruction: America’s Unfinished Revolution, 1863–1877* (New York, 1988), xxvii, xxiv–xxv.

¹⁰⁸ James D. Anderson, “How We Learn about Race through History,” in Kramer, Reid, and Barney, *Learning History in America*, 96–97.

¹⁰⁹ Robin D. G. Kelley, “‘We Are Not What We Seem’: Rethinking Black Working-Class Opposition in the Jim Crow South,” *Journal of American History*, 80 (June 1993): 78.

"P 905 .A512 × 100": An Ethnographic Essay

GREG DENING

"THE CROAKERS," William Sloane wrote on page 3 of the first issue of the *American Historical Review*, "have been saying that indulgence in generalizations must necessarily destroy thoroughness in detail." "Croakers," I thought, in ethnographic mode. "Surely an Americanism." But, being in historical mode as well, I consulted my 1811 *Dictionary of Vulgar Tongue and Buckish Slang, University Wit and Pickpocket Eloquence*, and there "croaker" was. "Croaker. One who is always foretelling some accident or misfortune: an allusion to the croaking of a raven, supposed ominous." Very ethnographic, I thought. Ravens, indeed. "Croakers: Raw and Cooked" flashed through my mind as a possible title for this essay, getting ravens and "blackbirds baked in a pie" momentarily confused. It would be nice, however, if the ethnographic truth of the *American Historical Review* was to be its "anti-croakerism."

At the time, I was sitting on the basement floor of the Baillieu Library, the University of Melbourne, *observing* a hundred years of P 905 .A512. It was one of those dark spots in the farthest corner of the library. Perhaps you know it well. The lights are eternally clicking off. The space between shelves is so designed that the human body can neither squat nor bend to see the bottom shelves. The nearest place to read and rest a book is a floor and a labyrinthine walk away.

I say I was sitting, but I was really moping. I had just lost a fight with the Borrowing Rights Librarian over my request to borrow any one volume of the *AHR* for more than three days at a time. No matter that I, an alien Stranger, had been invited to write an ethnography of one of the most prestigious historical journals in the world—to take more than one volume for more than three days was unprecedented and, worse, would set a precedent. No matter—[*Editor, AHR: I was fighting for my sanity and meant no harm*—no matter that the only evidence of any volume having been borrowed at any time was one that had "To Be Returned by the Last Date Shown Below"—27 July 1973. I had borrowed it myself. I had been writing an article called "History as a Social System" and was looking at the first volumes of all the great historical journals of Europe and the English-speaking world to see how they socialized their profession to think and write "historically." No matter, the librarian said, "somebody" might want them. But, for the sake of my concerns, he would take my case to the Privileges Sub-Committee of the Library Committee of the Council of the University.

So I sat moping, awaiting my fate. To be honest, I was observing very little: no rituals, no mythologizing, nothing ethno- at all. The color of the binding changed

circa 1970. Volume 1 was a Kraus reprint. (I wondered whether we colonists down-under were a little slow to subscribe. But no, I checked, and we were a founding subscriber. Volume 1 had been stolen, that's all.) I checked for "patterns of mutilation"—one of the phrases librarians use when they suggest to some poor lecturer that it is his or her students who are cutting out articles. Nothing out of American history; mostly, rather ominously, out of German twentieth-century history; no Presidential Addresses at all! I was wondering how long I should wait before thanking the Editorial Board of the *AHR* for their flattering invitation and saying sorry, it was beyond me.

What stuck in my throat was that I had recently heard an elder of the tribe of American historians address an assembly in Williamsburg, Virginia. "Ethnography is dangerous," he said in somewhat the same tone as I remembered General Jack D. Ripper in Stanley Kubrik's film *Dr. Strangelove* (1963) bemoan the imminent loss of his "precious bodily fluids." I thought that was probably a bit of a "croak." The Elder was not expressing sympathy for that brave band of professional strangers risking all in deserts, islands, and jungles for the sake of human learning. Or even for me sitting on the basement floor seeing my professional standing evaporating because I had agreed to do something that could not be done. No, he was foretelling the doom that would befall American history if it pursued ethnography any further. Ethnography distracted from the main event, he suggested, and put the trivial at center stage. We need to get back to basics, to politics, not rituals, to important people, not culture. It was as good a "croak" as I had heard in a long time. I decided to proceed dangerously.

WELL! THAT'S IT. I'VE DONE IT. That's how all the best ethnographies begin—with Evans-Pritchard in his tent suffering "Nueritis," with Clifford Geertz ducking from authorities trying to break up an illegal Balinese cockfight—locating the author in the observational process, warning that an ethnography of the "Nuer" is not the Nuer, an ethnography of the "Past" is not the past, of the "American Historical Review" not the *AHR*.¹ An ethnography will not end anything, any more than a history will. An ethnography is a sentence in a conversation we are having about something else. Sometimes there will be a response to that sentence. Sometimes there won't. Perhaps the happiest thing that could happen to it as a sentence is that someone will think, "That is just what I was about to say."

Discourse. Henry Adams, in a rather breezy effort at cliometrics, estimated in the first *American Historical Review* (1895) that every historian could be in error for one in five of every personal observation of historical facts. Presume, he wrote, that there is one fact per line in historical writing. A history like that of Macaulay's could be said to contain 150,000 assertions or assumptions of facts. "At least thirty thousand of these so-called facts must be more or less inexact."²

We have to assume, I think, that Adams was having his own "croak" about being scientific in history, maybe even a "croak" at that mild sort of science that William

¹ E. E. Evans-Pritchard, *The Nuer* (Oxford, 1940), 12–15; Clifford Geertz, "Deep Play: Notes on the Balinese Cockfight," in *The Interpretation of Cultures* (1973; London, 1975), 412–53.

² Henry Adams, "Count Edward de Crillon," *AHR*, 1 (1895–96): 51–69, quote on 51.

Sloane was advocating in his introduction to the first issue of the *AHR*.³ Sloane looked a little enviously at the advances that “anthropology, mythology, archaeology, physical geography, philology, psychology, and all their sisters, each in its own subdivisions,” had made. They had made their advances, Sloane thought, by their discovery of order, by their avoiding what Immanuel Kant had suggested they avoid, “circumstantiality.” Sloane does not say what the order is that history needs to discover. For him, it is more a confidence that there is some uniformity of human nature across time. This uniformity holds the “essentials” of human behavior. These “essentials” could not be reduced to any one, say the “mere political,” but are present in the “social, industrial, commercial, aesthetic, religious, and moral conditions of the common man.” They would reveal themselves in the “stirp” or the “continuous race-life” of a group. “Stirp”—“to use Galton’s phrase,” he says, as if contemporaries would easily recognize the concept. Maybe they would. Francis Galton’s *Hereditary Genius* (1869), *English Men of Science: Their Nature and Nurture* (1874), and *Natural Inheritance* (1889) were the core studies of eugenics. Galton had just invented (1892) the science of fingerprinting. The study of “stirps” or “race-life,” Sloane suggested, is an optimistic one. True, “the vital power or plasm” might be “dwarfed or crippled,” but it “will reappear with all its pristine strength and goodness in a later generation.” The proof of that? “Every reader will recall certain well-known convict colonies established several generations ago in different parts of the world which are now thriving, wholesome societies.”⁴ [*And that was only 1895, Mr. Editor. Look at us now!*]

Sloane had no strategy for this new science of history, but historians were categorizing their interests in new ways, to him one of the most positive signs that it was under way.

Middle-aged and older men will remember with some amusement the amazing historical charts which used to adorn the walls of schoolrooms, and resembled nothing so much as rainbow-colored rivers vaguely rising at the top, and wandering in viscid streams more or less vertically, according to the law of gravitation or the resistance of the medium, until absorbed one by the other, or lost in the ferule at the bottom. We rule our charts differently now; by straight horizontal lines, nearer or farther apart according to the period of general history with which we are concerned. The great stream is monotone, though not monotonous; and if it be but a single year that we study, we investigate it clear across, from where it scours the channel toward both shores, including even the annals of semi-civilized and barbarous peoples, so far as they seem to affect the current or the eddies. We have found the movement of the race more majestic than that of nations or individuals, the interest in man more intense than that in men or persons, and the development of civilization more instructive than the achievements of heroes.⁵

The great categories of this historical discourse were Ancient, Medieval, and Modern, with national subcategories where they were relevant. For a hundred years, the *American Historical Review* in its Reviews of Books, in its Historical News, in Documents Published and Bibliographical Details would sustain this categori-

³ William M. Sloane, “History and Democracy,” *AHR*, 1 (1895–96): 1–23.

⁴ Sloane, “History and Democracy,” 3, 2, 6, 7.

⁵ Sloane, “History and Democracy,” 4.

zation. As exhaustive knowledge of current research and, even more, knowing the "scuttlebutt" about what was to be published became important credentials for historical professionalism, the *AHR* would raise its services in this area to as much as 80 percent of its pagination.

Being totally familiar with what has been done and with what is being done across these categories is a costly form of discourse in terms of scholarly energies. It is comfortable in one sense and dispiriting in another. Dispiriting because there is so much that one will never read. Comfortable in that the categories of Ancient, Medieval, and Modern do not seem to matter so much. Occasionally, there are snide remarks across categories about the sort of thing medieval or ancient history does to the mind or about the low standards of scholarship the modernists have come to expect. But the categories are inclusive, and there is none of the pain that exclusive categories create. "Croakers" come out in strength whenever a prefix is added to history, psycho-, say, or ethno-. And sometimes there are giant fights between pre- and proto-. An adjective or a prefix before the word "history" seems always an effort to make history something else.

Being a historian was reaching some definition by the 1890s. Reduce partisanship, narrow the segment of inquiry, exhaust the sources, submit to acceptable criteria of reference and argument, progress by adversary proceedings, beware of the professional review, pursue the past as an end in itself not as a means of wisdom and legitimation. Prudence was the historian's real virtue. Prudence was something that could be taught. I have not seen the records of students through these years, of course. But let me guess at the sorts of things they might say. The students should learn to live easily with tentativeness in theory production. They should be satisfied with persuasion rather than analytic conclusions, with patterns rather than laws. They must put some premium on seeing events as whole rather than through some narrow perspective. They should discover the problems of history out of historians. In problem solving, they should be controlled primarily by the sources. They should have some commitment to romanticism in its attachment to the concrete, some commitment to idealism in the primacy given to human consciousness and verbalized social reality, and, finally, some commitment to a moderate relativism in its pragmatic definition of objectivity and in its acceptance of the intrusion of the historian's values.

Prudence is a form of consciousness, a craft sense of what is legitimate, a sense—not easily conceptualized—of being able to distinguish the right way and the wrong way of doing things. It is supported by all the ways in which boundaries are created around groups. Caricature, vituperation, exaggerated fears, emotional rhetoric, unsubstantiated generalizations are as common between disciplines as they are between race, religion, class, and gender. Prudence is taught, but it is also inculcated by professional associations and journals.

The *AHR* did not nail its editorial policies to the mast except in Sloane's idealism. Other national historical journals did. Mandell Creighton, the first editor of the *English Historical Review*, declared, "No allurements of style will secure insertion for a popular réchauffé of facts already known and ideas already suggested."⁶ By insisting from its beginning—not in so many words but in the

⁶ Mandell Creighton, "Prefatory Note," *English Historical Review*, 1 (1886): 5.

directions it took—that the word *American* in *American Historical Review* was not so much an object of historical inquiry as a description of the origin of it, the *AHR* did not have to *discipline* its local historians as brutally as other national reviews. The professionalism of most of the books under review was already established out of a European, especially German, tradition that was greatly respected. Reviews could be hard hitting, nonetheless. An unsigned review—(I noticed a number of unsigned reviews over the years. Whether this was editorial accident or not I could not tell. Often, they would be trenchant criticism of well-known historians!)—of Thomas Hodgkin's *Italy and Her Invaders*, stated: "It would have been most welcome to all such readers, if, instead of toiling through 1100 pages, they had been let off with 400, and the change could easily be made without losing a word that helps towards the result. Verbosity and what he styles the 'dignity of history' seem to mean almost the same thing to our author."⁷ William B. Weeden defends his repetition of criticisms of H. D. Traill's *Social England* that had already been made in the *English Historical Review*: "It is well to bring out the seamy parts of any performance, and to warn both readers and scholars against the deficiencies of any historical work."⁸

In my experience, it is not easy for professional historians to find a place for the considerations of "local" societies. There is plenty of evidence in the *AHR* in the reports of annual meetings of the tension between the "antiquarianism" of local histories and the "true" history of the professionals. But it is not something that flows into the reviews as it does in other national historical journals. Julian P. Boyd's "State and Local Historical Societies in the United States" gave me some understanding why.⁹ The vigor of historical consciousness in the United States, as Boyd tells it, is something to marvel at. I liked the thought of Jeremy Belknap's urging historical societies to be "keeping a *good look-out*, not waiting at home for things to fall into the lap, but prowling about like a wolf for the prey."¹⁰ I have had many reasons over the years in my Pacific studies to bless Belknap's propensities to collect. Perhaps Peter Augustus Jay's remark that "a file of American newspapers is of far more value to our design than all the Byzantine historians" is a little extravagant, but his spirit is right, too.¹¹ The realization that a migrant people have a history in their new lands and that "true" history is not just British, or Italian, or Renaissance history liberates the historical spirit greatly. Of course, in a society moving westward, "true" history might not be seen as that of the "old countries" but of the East or the "Atlantic," as can be seen in the sentiments expressed by Timothy Flint on the formation of the Antiquarian and Historical Society of Illinois: the opening address to the new society, he said, would "tend to remove the film from the eyes of those of our Atlantic readers, who still think, that

⁷ Review of Thomas Hodgkin, *Italy and Her Invaders*, vols. 5 and 6, in *AHR*, 1 (1895–96): 109–10.

⁸ William B. Weeden, review of H. D. Traill, ed., *Social England: A Record of the Progress of the People*, vols. 1–3, in *AHR*, 1 (1895–96): 125.

⁹ Julian P. Boyd, "State and Local Historical Societies in the United States," *AHR*, 40 (1934–35): 10–37.

¹⁰ Jeremy Belknap to Ebenezer Hazard, August 21, 1795, Massachusetts Historical Society, *Collections*, 5th series, vol. 3, pt. 2, 356–57, quoted in Boyd, "State and Local Historical Societies," 18.

¹¹ Martha J. Lamb, *History of the City of New York* (1880), 2: 515, quoted in Boyd, "State and Local Historical Societies," 19.

there is neither taste, oratory, or [*sic*] fine writing in the backwood's country."¹² The "[*sic*]" is Julian Boyd's, but no doubt many an Atlantic reader would have liked to have used it. I note in that respect that when the AHA inaugurated the Conference of Historical Societies, the first invitations were limited to the institutions of the South and West, because eastern societies "were not as a rule confronted by the questions which troubled those of the newer states."¹³ I dare say it would not have been the first or last effort to get things right when there was such prickly sensitivity to condescension and such condescension to prickly sensitivity. When there seems so much to be gained or lost by having a proper prudential attitude to historical inquiry, it is difficult to share the notion of "history" around or to have a sense of the vernacular nature of historical consciousness. It is not easy to agree to the notion that all the varieties of historical consciousness that fill our social and cultural lives should have their own poetics. If one admits that the others each have their poetic, there doesn't seem to be a reason why "true" history should not have a poetic, too. "Many historians find it easy to be historically minded respecting everything save only history." That is Charles Homer Haskins being quoted by Lewis Hanke in his 1975 Presidential Address.¹⁴

When the categories of discourse are as broad as Ancient, Medieval, and Modern, entry into the discourse becomes bland and subdued. Some field or event has been neglected.¹⁵ Some legend needs to be deflated.¹⁶ Sometimes a piece will begin with some visual effect: after 591, when Columban and his Irish monks came to Burgundy, they were followed for a century and a half by scores of *peregrini*. "Their foreheads shaven back to the middle of the skull, with long, stringing locks and painted eyelids, they streamed through Gaul," writes Helen Robbins Bittermann in "The Influence of Irish Monks on Merovingian Diocesan Organization." Her article ends, however, with the fairly modest conclusion that the changes the Irish made on Merovingian diocesan organization were an "indication rather than a cause of the decay."¹⁷ Sometimes the piece does not begin with much excitement at all. "While the importance of the part played by castles in feudal England has long been recognized by historians, comparatively little attention has been paid to the arrangements which were made for garrisoning these fortresses." It ends, however, just as modestly: "Although the sparsity of evidence makes dogmatic statements impossible and generalizations dangerous, I wish to offer a tentative summary of the history of castle-guard service."¹⁸ "Why has the history of the war in this segment of the frontier been neglected?" asks

¹² Timothy Flint, *Western Monthly Review*, 1 (1828): 563, quoted in Boyd, "State and Local Historical Societies," 23-24.

¹³ Frederick Wightman Moore, "First Report of the Conference of State and Local Historical Societies," *AHA Annual Report* (1904): 221, quoted in Boyd, "State and Local Historical Societies," 29.

¹⁴ Lewis Hanke, "American Historians and the World Today: Responsibilities and Opportunities," *AHR*, 80 (February 1975): 10.

¹⁵ Charles M. Andrews, "Colonial Commerce," *AHR*, 20 (1914-15): 43-63.

¹⁶ Archibald Henderson, "The Creative Forces in Westward Expansion: Henderson and Boone," *AHR*, 20 (1914-15): 86-107.

¹⁷ Helen Robbins Bittermann, "The Influence of Irish Monks on Merovingian Diocesan Organization," *AHR*, 40 (1934-35): 232, 245.

¹⁸ Sydney Painter, "Castle-Guard," *AHR*, 40 (1934-35): 450, 459.

Julius W. Pratt in his "Fur Trade Strategy and the American Left Flank in the War of 1812."¹⁹ Largely, he answers, because there were few men engaged in it, it had no influence on the peace, and there had been no documentation. Some papers in the War Department and the Indian Office had recently been "discovered."

Discourse in these circumstances has a mechanical feel. It does not cost much in the sense that it does not ask for any gamble, any imprudence. To be something else, to be practitioners of a science in William Sloane's ambition, historians would have to be at least temporarily imprudent. Science is the gamble that if one blinkers oneself in a particular way, one will see the "real" beneath the surface of things. In a science, one "sees" cell structures, psyches, neutrons, black holes, etc. "Seeing" these takes much discipline and much politics. Seeing them is more a form of consciousness than a logical system. It is, as much as anything else, determining what impressions in a world of human experience are worth one's attention. It is a question of balance. Something is lost in this blinkered view for the gain. Of course, there are plenty of examples in which the sense of blinkering is lost and the model of reality that the science scans is mistaken for the reality itself. That is the dangerous moment when philosophers become kings.

There was a time when the *AHR* and the profession seemed to shiver a little at the thought of the cost of a discourse that would be less than prudent. It was in 1934, the fiftieth anniversary of the establishment of the AHA. There was much reflection in the air. But it was also the year after Charles A. Beard's Presidential Address at Urbana: "Written History as an Act of Faith." H. E. Bourne, the editor of the *AHR* and the chronicler of the fiftieth anniversary meeting, remarked that Beard's lecture had stirred many to "reconsider their philosophy of history, or, perhaps, to look about and discover if they had one."²⁰ Beard's *Contemporary America, 1877-1913*, had received scant praise in an unsigned review in 1914. "Poorly grounded, unauthoritative and far too restricted in point of view."²¹ In 1934, Theodore Clarke Smith was even more strident in his fears. Beard wanted to throw away the Rankean formula and replace it with philosophy, he said. That was a formula for thought control, Clarke felt. He believed that another fifty years would see the "final extinction of a noble dream, and history, save as an instrument of entertainment, or social control will not be permitted to exist." He hoped that if the AHA were then disbanded, "those of us who date from what may then seem an age of quaint beliefs and forgotten loyalties, may go down with our flags flying."²² Maybe William Sloane was saying "croaker" from the grave.

We are not as far from Henry Adams and his breezy cliometrics as you might think. Remember, Henry had been happy enough to concede that there were thirty thousand errors in Macaulay. In 1975, another cliometrician, more famous for his cliometrics by far than Henry Adams, Robert William Fogel, contributed his thoughts on "The Limits of Quantitative Methods in History." The writing of *Time on the Cross* with Stanley Engerman, he witnesses, "has been a chastening

¹⁹ Julius W. Pratt, "Fur Trade Strategy and the American Left Flank in the War of 1812," *AHR*, 40 (1934-35): 246-73, quote on 246.

²⁰ H. E. Bourne, "The Fiftieth Anniversary Meeting," *AHR*, 40 (1934-35): 423-38, quote on 425.

²¹ *AHR*, 20 (1914): 179-80.

²² Theodore Clarke Smith, "The Writing of American History in America, from 1884 to 1934," *AHR*, 40 (1934-35): 439-49, quotes on 449.

experience for us. In the course of these labors, our views of the relationship between science and humanism in the writing of history have evolved significantly . . . We have come to recognize that history is, and very likely will remain, primarily a humanistic discipline. We now believe that the issue raised by historical quantifiers is not whether history can be transformed into social science but the realm of usefulness of social-science methods in a humanistic discipline." There is an important distinction, he writes, "between *being* scientific and *making use* of scientific findings."²³ He is correct, I think. The cost of *being* scientific for most historians is too great, but *making use* of scientific findings is to integrate into history what we do in living, readjust the disadvantages of blinkered viewing.

That readjustment of the blinkered views that intrude on our everyday cultural existence belongs very much to our twentieth-century experience. The twentieth century for historians has not been so much the Death of the Author as the Birth of the Reader. Suddenly, we know that our most vital social and cultural skill is our ability to read—that is, not just to be literary but to see, hear, touch, smell all the signs around us with political astuteness. The real world we live in is one in which we have to distinguish the many meanings of apparently simple signs, to match the tropes of all the narratives around us to the occasion of their telling, above all to catch the politics—the empowering and disempowering forces—that suffuse all our knowledge and representations. Suddenly, whatever our analytical and theoretical propensities, we are culturally postmodern. We might not have the labels—fiction, trope, metaphor, metonym—to be semiotically descriptive of how we read, but our cultural existence depends on our everyday ability to recognize the reality behind the labels. That is the present cultural and disciplinary context of any historian's discourse.

Presenting the Past. Volume 40 of the *AHR*, I have to confess, depressed me a little. There was altogether too much "croakerism" in it. Volume 60, 1954–55, on the contrary, exhilarated me. If, in its centennial reflections, the *AHR* were tempted to ask what volume or what issue realized the journal's high ideals, then I would nominate Volume 60. Let me tell you why. Merle Curti's Presidential Address, "Intellectuals and Other People," is in Volume 60, for one thing.²⁴ That is a grace in itself. Every sentence of the address is stamped with the elegant courtesy and sensitive wisdom of that great man. More than that. This is 1954. The McCarthy crisis is upon the American polity. We do not need to use refined statistical methods, Curti wrote, "to know that fear is abroad in our country and that those who live by ideas are especially subject to hysterical and unwarranted attack." Those who know Curti in his work will know that John Dewey was one of his great inspirers. On this occasion, the inspiration comes from Dewey's rejection of any dichotomy between intellectual life and social action. "Thinking indeed is activity, symbolic activity, and an idea is an embryonic act." The social action of the intellectual is criticism, a commitment to "freedom of thought and expression

²³ Robert William Fogel, "The Limits of Quantitative Methods in History," *AHR*, 80 (April 1975): 329–50, quotes on 342.

²⁴ Merle Curti, "Intellectuals and Other People," *AHR*, 60 (1954–55): 259–82.

in its widest scope.”²⁵ And criticism must not just be of the myths that control the past but of the myths of every description that infuse the present.

It is the function of the Presidential Address to be reflective, to make things explicit. The annual appearance of the Presidential Address makes the *AHR* special among the great professional historical journals. The presidents will mostly disclaim their capacity to make sense of such complex issues and are always wary of having to give witness to their most honest thoughts before their peers. Such declaratory contemplation is too much like “theory” for most of them. Most would just like to tell their story and run. Most would probably agree with that wise counselor of souls, Thomas à Kempis: “I would rather feel compunction of heart than know how to define it.”²⁶ “I would rather write history than talk about it.”

I suppose that is what made Volume 60, 1954–55, so exhilarating for me. There is not an article in Volume 60 that does not address in some way the dangers to which Curti was referring. They all *present* the past in the sense that their narratives about the past address the issues that concern them in their present. I dare not use the word “relevance.” The word has come to contain too many clichéd and trivialized presumptions. Above all, as we experience the demand to be “relevant,” it seems to mean that something is only “relevant” if it is explicitly and consciously displayed. Volume 60 is not exhilarating because each of the authors spells out the “relevance” of his topic for the present. On the contrary, they participate in the highest form of historical art by creating such theater in their narrative that any reader will understand that their story is really about two worlds, one that is past and one that is present.

So, an unlikely collection of diverse pieces is “emplotted” (in the Aristotelian sense) by liberating the readers to unite something about which they might have little expertise with something about which they have considerable expertise, their own living. Each narrative becomes a parable of something else: “Iconoclasm during the French Revolution” (Stanley J. Idzerda) about the ways in which political systems can become immured in museums and the ways in which systems can be self-contradictory and still win; “The Treason of Sir Roger Casement” (Giovanni Costigan) about the ways justice is manipulated to political ends; “The Teapot Dome Scandal and the Election of 1924” (J. Leonard Bates) about the impossibility of isolating morality to one side or the other in political debate; “European Liberalism in the Nineteenth Century” (David Harris) about the ways in which individual rights were protected by delaying the quick actions of tyrannical executives; “Bismark and German Nationalism” (Otto Pflanze) about the ways in which the chimera of national unity creates monstrous regimes; “Anarchism and the Assassination of McKinley” (Sidney Fine) about the ways communism rather than anarchist propaganda of words became constituted as the most important threat to national security; “The Ethical Revolt against Christian Orthodoxy in Early Victorian England” (Howard R. Murphy) about the ways abstract theory has social origins.

The theater of history is not all tragedy. There is comedy, too. There must, in 1954, have been a satisfying sense of irony for a reader to turn to Oron James

²⁵ Curti, “Intellectuals and Other People,” 277, 279, 282.

²⁶ Thomas à Kempis, *Imitation of Christ*, I, iii.

Hale's Notes and Suggestions on "Adolf Hitler: Taxpayer."²⁷ There is a touch of prurient enjoyment in reading a dictator's tax file. Hitler had to squirm like the rest of us. Listen to him trying to make a tax deduction out of all his political activities because he was the writer of *Mein Kampf*.

Out of the income from my book, for this period, only a very small fraction was expended for myself; *nowhere do I possess property or other capital assets that I can call my own*. I restrict of necessity my personal wants so far that I am a complete abstainer from alcohol and tobacco, take my meals in the most modest restaurants and aside from my minimal apartment rent make no expenditures that are not chargeable to my expenses as a political writer . . . *Also the automobile is for me but a means to an end. It alone makes it possible for me to accomplish my daily work.*²⁸

No, said the tax office. These are not professional expenses within the meaning of the law. But because it "provides material for his work as a political writer, . . . one half of the claimed expenditure for travel and salaries is allowed as a deduction from income."²⁹ I wonder what William Appleman Williams, being persecuted politically as he then was by the IRS, thought when he read that.

"Presenting the Past" can mean a number of things. It has the theatrical connotation of *presentation*, of transforming the past into a dramatic unity. It has the socio-temporal sense of bringing the past into the present. The social and cultural forms of everyday life—gender, age, class, ethnic grouping—are made in the narratives we make of the past. Our present moments are made by making sense of what has happened. Finally, "presenting the past" can mean writing about the past as if it were a series of present moments, that is, not endowed with the hindsight clarity of the moment after but still containing all the possibilities to come. I call this sort of history writing "present-participled"—not life but living, not science but "sciencing," not culture but "culturing," not gender but "gendering." Historians have to share the uncertainties of any present moment in this sort of writing. Divesting oneself of certainty is an uncomfortable mode of writing history, and sometimes embarrassing when it is certainty that gets the kudos—and the research grants. I think Merle Curti when he exhorts us in his Presidential Address to avoid the overemphasis of "vocationalism" and calls on historians to revive the critical function of liberal education is urging all to temper the certainties of being scientific with the ambivalence of being human. I will not burden the authors in Volume 60 with either my prejudices or my vocabulary, but I think their volume "works" not because it was "relevant" to their times but because they had the courage to write history as if it were a parable and the courage to be humble about what they did.

An Archaeology of an Ethnography. In any archaeology of knowledge, the wrong place to be is on the basement floor. In this sort of archaeology, the only place to be is on top of the deposit. The view is better, for one thing; but, more important, one knows that one is so high only because of all that is beneath. A hundred years of accumulated knowledge is a humbling sight. It won't take a hundred years to read, but it will seem so. I seem to remember that Henry

²⁷ Oron James Hale, "Adolf Hitler: Taxpayer," *AHR*, 60 (1954–55): 830–42.

²⁸ Hale, "Adolf Hitler," 832–33, italics in original.

²⁹ Hale, "Adolf Hitler," 833.

Kissinger said, when he heard that Richard Nixon had everything that happened in the Oval Office taped for history's sake: "Eight years of tape will take eight years to hear." Mr. Editor, I did not know how to do what you asked me to do other than digging exploratory shafts into the deposit. I would try every twenty years, I thought, with no real reason for being so metric in my choice. I presume one reason for your compliment in inviting me to write an ethnography of the *AHR* was because I had the ethnographic advantage of being the Professional Stranger. Roy Wagner, as you may know, turned the ethnographer's world upside down by suggesting that "culture" is an outsider's invention.³⁰ But Pierre Bourdieu turned it right way up again by showing that such a "culture" inevitably comes to be described as a map. The outsider finds a way around a foreign landscape by use of a model of all possible routes. The "native," on the other hand, has a sense of journeys actually made and brings all the potential axes of space into a practical relationship with the traveler's body.³¹

My method left me with scraps of maps and the depressing realization that even if I invented a way in which the scraps were related to one another, I still would not have fulfilled my ethnographic ambition and described how a hundred years of the *AHR* actually happened. Mr. Editor, I have not the years to do that. I am sure, however, that when the celebratory balloons have settled, someone will do that for you. That, surely, is the one certainty we historians have. There is someone coming who will tell us what we really said when we appeared to be saying something else.

Of the many assumptions that I had at the beginning, there was one that I thought would serve me well. It was that we all find it difficult to plumb the depths of our own plagiarism. It is a blow to one's self-esteem to read through a hundred years of scholarly endeavor only to find that so much of what one thinks one has "discovered" has been said before. I find myself offended at the sort of historiography that belabors past historians for their simplicity or their errors. Mostly, these sorts of critics punish the past for not being what these critics now are because of the past. But Roland N. Stromberg in his "Some Models Used by Intellectual Historians" challenges this model of what he calls the "immanent dialectic." Or what Arthur O. Lovejoy called "Platonic dialogue on a grand scale."³² But then I do not think I am proclaiming some great progression of thought outside our control. For me, the mystery of describing a hundred years of knowledge lies in the question, "What changes? The issues of human behavior or the language in which they are described?" I wish I could be more helpful in giving an answer.

³⁰ Roy Wagner, *The Invention of Culture* (1975; rpt. edn., Chicago, 1980).

³¹ Pierre Bourdieu, *Outline of a Theory of Practice*, Richard Nice, trans. (Cambridge, 1977).

³² Roland N. Stromberg, "Some Models Used by Intellectual Historians," *AHR*, 80 (June 1975): 563-73; Arthur O. Lovejoy, *The Revolt against Dualism*, 2d edn. (New York, 1960), x, quoted in Stromberg, 563.

Featured Reviews

FLORENCIA E. MALLON. *Peasant and Nation: The Making of Postcolonial Mexico and Peru*. (A Centennial Book.) Berkeley and Los Angeles: University of California Press. 1995. Pp. xxiv, 472. Cloth \$55.00, paper \$19.00.

In an era of historiographical Balkanization, Florencia E. Mallon's important book offers a broad paradigm tying together cultural, social, and political approaches to the past, as well as local and national perspectives. It is a book with a mission: not only to demonstrate how Latin America's modern history ought to be written but also, in so doing, to rescue and revitalize the lost, distorted, or repressed alternative visions and projects of rural subalterns. Mallon does this through painstaking scrutiny of local politics in two Mexican and two Peruvian regions during eras of crisis, and framing it in an ambitious theoretical approach.

Mallon's central thesis is that the actions and political projects of peasants and other rural subalterns decisively shaped the processes and outcomes of nation-state formation in Mexico and Peru during the period of Latin America's "national democratic revolutions," long, drawn-out political, economic, social, and cultural transformations roughly spanning from the late-colonial period to the 1930s (longer in Peru). The contests over competing national projects became particularly intense during periods of "rupture," brought about by international or civil wars. For Mexico, the author analyzes the years between 1853 and 1876, an era of stormy liberal reforms, civil wars, the French intervention, and the restored republic. For Peru, she focuses on the devastating loss to Chile in the War of the Pacific (1879–84), subsequent civil wars and local insurgencies, up to the early years of the "Aristocratic Republic" (1895–1919).

Mallon pictures the struggle over the nation-state as a vast and intricate mechanics, characterized by continuous processes of coalition-building and fragmentation or conflict, from inside Indian peasant communities up to the national centers of power. Using a reworked Gramscian notion, she terms these processes "hegemonic": the contestation, legitimation, and redefinition of power and meaning by groups defined by class, race, and gender at all levels of social

organization. By exploring political domination as the outcome of both repression and consent, power and meaning, interests and discourses, surveillance and solidarity, Mallon hopes to link issues of class conflict with cultural issues.

The author's two Mexican cases are situated in the central and eastern Sierra de Puebla and in the state of Morelos. In the Puebla highlands, with a historically weak sector of landed estates, many of the ethnically diverse peasant communities adopted a popular version of liberalism during the Wars of Reform. Between 1862 and 1867, during the bitter struggle of Liberals under Benito Juárez against the French intervention, the militancy and radicalism of the village liberals reached its peak. Under the influence of younger peasants enrolled in the National Guards, the longstanding communal "patriarchal reciprocity" was transformed into "democratic patriarchy." Popular or communitarian liberalism eschewed the notion of individual property rights, stressed the rights of the communities to their land, local autonomy, and popular education, and used bravery and merit in the struggle against the enemies of the nation as the basis of entitlements for the citizens of the Mexican nation. After Emperor Maximilian's defeat, Puebla's serrano communities were increasingly excluded from the politics of the restored republic, as Juárez and the elite Liberals needed to centralize power. In this climate, Porfirio Díaz's challenge to the Juárez faction represented the popular liberals, now betrayed by their erstwhile comrades in arms. Only later did the porfiriato turn into a purely repressive regime and thwart the lasting incorporation of subaltern popular political projects necessary for "hegemonic outcome." But in Puebla the foundations of peasant counterhegemonic projects had become so strong during two periods of rupture between 1810 and 1867 that they survived and resurfaced in the Revolution of 1910. In the case of Morelos the peasants faced a more formidable challenge from

powerful owners of sugar estates. The peasants entered a broad regional popular liberal movement that precipitated the Reforma of the 1850s, but they became disoriented a few years later when the moderate liberal government in Mexico City sent troops to repress them. By the 1860s they were ready to respond to more hierarchical Conservative populist overtures by Emperor Maximilian, only to discover that these were empty promises.

Mallon's briefer discussions of her two Peruvian regional cases take on a more somber tone. In one of them she revisits the setting of her previous monograph on the economy, society, and politics of the Indian peasant communities in the Mantaro Valley, in the central Andean department of Junín (*The Defense of Community in Peru's Central Highlands: Peasant Struggle and Capitalist Transition, 1860–1940* [1983]). The Mantaro Valley shared with the Sierra de Puebla a weak entrenchment of large estates and a strong tradition of Indian peasant communities and mestizo smallholders well integrated into trade networks with the nearby capital, Lima. After Chile's victory over Peru's regular army in 1881, General Andrés Cáceres organized a guerrilla campaign—centered in the Mantaro Valley—to harass and dislodge the foreign invaders. His forces consisted largely of village-based irregulars who identified the patriotic struggle against the Chileans with their own goals of greater autonomy and control over communal lands. After the departure of the Chileans, Cáceres's policies gradually shifted from "populist impulses" to the "othering" and total exclusion from the body politic of his erstwhile peasant allies. Where autonomous hegemonic processes survived this Cacerist betrayal, they were finished off after 1895 by the regime of the founder of Peru's modern state, Nicolás de Piérola, and his successors. By combining repression with a kind of benevolence that construed the Indian peasantry as isolated and passive, the Piérola regime achieved "neocolonial reunification rather than national consolidation" (p. 266).

Mallon's second Peruvian region—the northern highland zone around Cajamarca—is the "limiting case" of her model, for here neither landlords nor peasants developed a "national project" in the aftermath of Chilean occupation. A region dominated by large livestock estates, here the two guerrilla movements of the 1880s were merely antistatist coalitions, fighting against taxation, army recruitment, and commercial penetration by powerful outsiders. Peasants joined both movements, led by petty merchants and large landholders, in a clientelistic fashion for want of more autonomous alternatives. In the end, with Piérola's consolidation of the national state after 1895, "a system of private landholder power" reemerged that "looked a great deal like traditional gamonalismo [Andean rural bossism]" (p. 242).

In both countries the coercive oligarchic regimes constructed in the late nineteenth century rested on ruling coalitions centered on "entrepreneurial land-

owning classes in combination with foreign capital," which required "the violent repression of social movements and popular resistance" (p. 273). But in early porfirian Mexico, popular elements continued to be noticeable in the ruling coalition, and although they disappeared "somewhere along the line" in favor of more exclusively entrepreneurial coalitions, in 1910 the grizzled old survivors of the battles against the French intervention and the Conservatives formed a living link to Reform-era counterhegemonic projects. In Peru, by contrast, "Piérola rebuilt the state on the corpse of the nineteenth-century popular movement" and strove to have the nation "composed of a few good men" (p. 275), an exclusivism that bred political fragmentation and clientelism and impeded the "consolidation of a truly national state."

The least satisfactory chapter of the book deals with local intellectuals, popular culture, and memory. Once "hegemonic outcome" was achieved in Mexico, the local intellectuals became brokers between popular discourse, memory, and interests and the national alliance in power. This mediation involved the partial repression and warping of local popular memories and interests. The local intellectuals of Puebla's most militant reform-period village ended up hiding important parts of its counterhegemonic struggles, defending state-controlled education programs that undermined the interests of local peasants, and consenting to the "othering" of the villagers by the Mexican hegemonic state during the parades commemorating the nation's heroic struggle against the French intervention. As we might expect, local intellectuals of Peru's Mantaro Valley communities were never even asked to "mediate" by Peru's fragmented non-hegemonic state. When in 1982 the community of Comas finally was recognized for its contribution to the resistance campaign against the Chileans, it was too late. Five years later the Maoist guerrillas of Sendero Luminoso involved the local peasantry in another civil war. Local intellectuals in the Mantaro Valley remained distrustful of a "hesitant national state" that had not improved their communities' material conditions during an entire century.

Mallon's challenging work combines theoretical sophistication with detailed empirical exemplification. Rather than the beginning of Latin America's "new history," as a blurb on the book's back cover proclaims, it represents an ambitious balancing act, the attempt to join recent approaches centered on culture with a heterodox Marxism focusing on class struggle and the nature of bourgeois-democratic revolutions. The author did not succumb to the siren calls of those practitioners of discourse analysis or Foucault-inspired historiography who proclaim the impossibility of tracing any "great arches." Mallon clearly believes that it is possible to do both: "de-center" the study of social and political conflicts, thus allowing the discourses, meanings, and interests of infinitely divisible groups of subalterns to be heard, while at the same time connecting them to, indeed

making them constitutive of, a central “story line” of nation-state building, and all the economic, social, and cultural transformations that entails. What is admirable about this work is Mallon’s relentless probing of local power constellations, her ability to tease different political and cultural projects out of the inevitably scant record left behind by peasants and local authorities. She succeeds in demonstrating the complexities and local contingencies of political alliances and projects, conditioned by longstanding cultural traditions, the history of class and gender constellations, and political institutions.

But in a number of ways the book poses more problems than it solves. The very notion of hegemony is so flexible, so spongy, that its heuristic value may be limited to the fundamental but rather obvious insight that stable rule requires both coercion and consent. In a recent book on state formation and revolution in Mexico edited by Gilbert Joseph and Daniel Nugent (*Everyday Forms of State Formation: Revolution and the Negotiation of Rule in Modern Mexico* [1994])—virtually a companion volume to Mallon’s, concerned with the same issues, relying on the same theoretical approaches, and with contributions by Mallon herself and many of the authors she cites most frequently—Derek Sayer remarked on the perplexing variety of meanings given to “hegemony” and drew the very notion of “hegemonic project” into question as a reification. What is the degree and nature of inclusion, solidarity, and consent in political regimes required to qualify them as exemplars of “hegemonic outcomes”? Do we not have numerous examples of political regimes in Latin America and elsewhere—Juan Perón’s Argentina comes to mind—that managed to be popular, advance certain social legislation favored by subaltern groups, and which were nevertheless marked by an extraordinarily high level of repression? Such questions point to the artificially polarized nature of the notion of hegemony: regimes are either hegemonic or they are not, and the latter presumably means that they are non-popular, purely repressive. Although Mallon presents us with a highly differentiated view of hegemonic *processes*, contingent on varying regional and local cultural, political, and socioeconomic matrices, ultimately everything seems reduced to this stark polarity: the struggles between various projects, between the interests and discursive strategies of distinct localized social groups either facilitate a hegemonic *outcome* or they do not. In the final analysis, above and beyond all the differentiation, Mallon presses her protagonists, from indigenous peasants to the elite contenders for national power, into this black-and-white pattern of hegemonic projects. On this level many of her finely tuned interpretations cede to judgments that are often starkly simplified or skewed.

Clientelism and patronage, for example, are crucial elements of Latin America’s post-independence political culture—as they were, in other guises, in New York City or Chicago—yet they are reduced by Mallon

to the maneuverings of ambitious elite politicians and hapless subalterns too weak to develop a counterhegemonic project. Clientelism is portrayed as a hallmark of Puebla’s pro-Juárez “elite-liberal” faction, in stark contrast to the popular liberals’ selfless dedication to their cause. This strikes me as unrealistic. Latin America’s political cultures have been marked by an overlap between the gradual development of preponderant local political orientations—the “tincturing” of the political landscape, as Guy Thompson has called it—and the reliance on strategies and assumptions about the polity shared by all actors, conservatives and liberals, peasants and professionals. Although Mallon discusses what might be called dialectic ambiguities—projects being simultaneously “counterhegemonic” and “hegemonic,” depending on the power arena concerned—she neglects the frequent appearance of ideologically ambiguous political movements in Latin America. Elite and subaltern groups alike have combined liberal planks with corporatist, state-interventionist, or purely clientelistic notions, the result of deep historical layers in the formation of political cultures that hark back to colonial and pre-colonial times. Failure to incorporate this fundamental feature of Mexican and Peruvian political cultures also leads Mallon to underestimate the importance of Catholicism. Rather than declining, the influence of popular religiosity on politics was growing during the late nineteenth and early twentieth centuries in many parts of Latin America, as the Catholic church sought to recover lost ground by addressing contemporary issues through lay movements. This influence cannot be minimized by describing it as a mere “entry point” to the material interests espoused by popular movements.

In short, in her analysis of the various political projects, Mallon’s balancing act between meaning and interests or power occasionally seems to break down. “Meanings” at times become mere “entry points,” or follow rather neatly from the power constellations and perceived interests of peasants and elite politicians. Democratic patriarchy in Puebla’s communities is interpreted to entail communitarian visions of landholding, when in fact the communities’ liberalism contained the tension between notions of individual families’ private property rights and the defense of the communal domain against outsiders. On the side of elite politicians, Mallon asserts the necessity to be repressive in the aftermath of periods of rupture for putting together stable ruling coalitions without sufficiently exploring the ideologies and presuppositions informing their actions. Class positions in her analysis often seem to determine the actions and political projects of social groups to a greater degree than the notion of political culture warrants.

Mallon’s insistence on “hegemonic outcome” seems driven in part by her comparative approach, her ambitious goal of explaining the different politi-

cal trajectories of Mexico and Peru. In this regard her interpretation renders disappointing results. Nobody would deny that during much of the twentieth century Mexico has been blessed with a more stable, more legitimate regime than Peru. But it appears reductionist to explain this difference with one overarching dichotomy: pure repression in Peru versus "hegemonic outcome" through partial inclusion of popular movements in Mexico. There is considerable evidence that even before the onset of the "national-democratic revolutions," during both the pre-hispanic and the colonial periods, Mexico had been characterized by more effective centralizing polities. Moreover, because of the location of the capital, Mexican political elites tended to be more engaged with the issues having to do with peasant communities than their counterparts in Lima. Most importantly, during the nineteenth and twentieth centuries Mexico has been affected more intensely and more frequently by foreign powers, especially, of course, the United States, than Peru has been. It is surely no coincidence that Latin America's two most stable twentieth-century revolutionary regimes, Mexico and Cuba, held sway in those countries most affected by the economic and political penetration of the United States. In the case of Mexico, the United States contributed to the stability of the postrevolutionary regime not only by providing legitimacy to the governing party's nationalist rhetoric but also by propping up its economy through capital investments, markets, and absorbing millions of Mexico's unemployed—not unlike the contribution of the original European Common Market to political stability in several southern European countries. All of these factors help to explain why in the history of the Mexican state the capital often has exerted more effective political control over the countryside than its counterpart in the central Andes. The "hegemonic outcome" of the Mexican revolution contributed greatly to this result for roughly half a century (from the mid-1930s to 1982), but even during that era other factors played an important role for regime stability and legitimacy.

It would be hard to argue with Mallon's point that many of Peru's governments in the century since the War of the Pacific have assembled an awful record of repression against rural and urban popular movements. But it is equally hard to argue that prior to 1910, during the era of oligarchic regimes that followed the periods of rupture in both republics, repression of such movements was harsher, let alone more systematic, in Peru than it was in Mexico. Mallon recognizes this and so her argument about different regime outcomes—hegemonic in Mexico, largely repressive in Peru—is, to a degree, *ex post facto*: conclusions about the nineteenth-century episodes of rupture and their immediate aftermaths deduced from her understanding of the twentieth-century record of both republics.

Yet key issues of Mallon's interpretation of Peru's nineteenth and early twentieth century are open to question. It is true that liberalism sunk deeper roots in Mexico than it did in Peru. But the author considerably underestimates its force in the Andes when she suggests that "liberalism as a revolutionary ideology" there "would gain little influence . . . beyond the ideas and writings of a few educated individuals" (p. 17). Liberalism was widely disseminated among peasant communities from the southern highlands to the north coast prior to the War of the Pacific and informed several rural revolts and confrontations. And the repression in the decades following the War of the Pacific did not simply wipe out autonomous movements in the countryside. In fact, it is impossible to understand the broad mobilization of highland communities during the 1910s and 1920s, influenced by progressive liberals, anarchists, and various and sundry *indigenistas*, without understanding the peasantry's social and cultural transformation between the 1820s and 1890s. In the early years of this century, Lima's governments ceased giving automatic support to the *gamonales'* offensive against Indian peasant communities and began to send commissions to the highlands to investigate complaints against abusive *gamonales* and to pass protective legislation for labor tenants and communities. Indeed, after the 1820s, Peruvian governments never again adopted a systematic policy to privatize communal lands as Mexican governments did between the 1850s and 1890s.

All this points to a relationship between social change, cultural continuities, and political regime change in Peru rather different from that found until the early twentieth century in Mexico: as Peru's political class appears more disconnected from its fragmented society, social changes appear to work themselves out—both through violent movements and through more piecemeal structural change—before the political system fully reflects those changes. It could be argued that in Mexico political changes at the center have facilitated social changes to a greater degree, from the struggle for independence through the liberal Reforma to the Revolution of 1910. If this is correct, then such a fundamental difference between the two nations' political cultures would require a more multifaceted conceptualization than the concept of hegemony.

Mallon's book combines brilliant insights with an exceedingly stark conceptualization of divergent paths of nation-state formation. The issues she raises will be discussed for years to come among historians of Latin America and other world regions. Whether or not one agrees with her views, she has courageously tackled central questions for our modern world with great skill and resoluteness.

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DAGMAR BARNOUW. *Critical Realism: History, Photography, and the Work of Siegfried Kracauer*. (Parallax: Re-Visions of Culture and Society.) Baltimore: Johns Hopkins University Press. 1994. Pp. xii, 350. Cloth \$60.00, paper \$19.95.

Do historians understand the historicity of what they do? If one reads Dagmar Barnouw's interesting book, it becomes clear that recent debates on epistemological questions reveal not a certain lack of self-consciousness about historical method but rather a lack of self-consciousness about what many historians have done all along, about the diversity of opinion that existed and still exists in the discipline, and about the impressive provisionality of historical scholarship as such. A film critic, journalist, philosopher, and associate of leading Weimar intellectuals such as Theodor Adorno, Siegfried Kracauer is perhaps best known for his seminal but flawed study of the history of German cinema, *From Caligari to Hitler: A Psychological History of the German Film* (1947). But Kracauer also wrote systematically about photography and, more to the point here, about the relationship between "camera reality" and historical reality. A probing extrapolation from Kracauer's ideas on this subject, particularly as they were developed in his posthumous *History: Last Things before the Last* (1969), argues Barnouw, suggests that the prolific Weimar journalist made significant contributions not only to cultural modernity but also to current debates on "the objectivity question" and the "return of literature" in historical study.

Expressing his views in a letter to Leo Lowenthal, Kracauer believed that the "historian has traits of the photographer, and historical reality resembles camera reality" (pp. 17, 59). Historiography preserves and creates cultural memory by "redeeming" past actions, in Kracauer's estimation, just as photography and film record images that make present what has become past at the moment they are captured. Although both the historian and the photographer order and shape the reality they articulate, they cannot completely overcome the phenomenal world from which they draw their material. That material is "real" because it is sharable; it contains patterns because it was or could be experienced from a multiplicity of not quite incommensurable viewpoints, the ordering of which represents the historian/photographer's special task of adjudicating between control of the document/image and submission to its "factuality." This adjudication situates history/photography "between" opinion and theology, between subjective reactions and the "higher" truths of, as Barnouw writes, "systematizing, generalizing predictive statements of cultural values concerning knowledge and representation" (p. 19). Both "art with a difference" and "science with a difference," history/photography for Kracauer becomes a provisional and indeterminate search for partial, temporary, and sharable truth about historical and physical reality.

Barnouw's complex discussion has much to say about Kracauer's contributions to film and photography theory; his intellectual and personal relationship with Adorno, Ernst Bloch, Bertold Brecht, and other luminaries of the Weimar intellectual scene; current discussions on the nature and status of visual representation in contemporary culture; and the relationship between memory and history in cultural modernity. But for historians, the most interesting and challenging sections of this book deal with Kracauer's critical realism, his belief in the historian's simultaneous dependence on the factuality of historical reality and consciousness of the provisionality of all historical truth. For Kracauer, history was never about fixed and immutable truths or about unburdened imagination and free play, never about "science" or "art" per se. As an exile, he was fascinated with the issue of the historian's temporally and culturally mobile self, which gained partial understanding of the past through historically specific interventions that over the course of a career produced historically specific selves interacting with others in an acknowledged, professional community of diverse knowing subjects. Accepting Kracauer's emphasis on the temporality of self and knowledge, but simultaneously stressing the existence of sharable information about the past, argues Barnouw, results neither in an endless deconstruction of meaning, as advocated in a provocative article by David Harlan in this journal ("Intellectual History and the Return of Literature," *AHR* 94 [June 1989]: 581-609), nor in a reassertion of timeless objectivity, which for the majority of historians, I would argue, has been seen as the utopian goal it is. In Barnouw's view, however, it should also not result in the reassertion of historians' practice of reconstructing the past according to accepted standards of professional scholarship if this reassertion overlooks the historicity of the practice and of the language use on which it is based.

Instead, Kracauer's idea of the "anteroom" status of historical knowledge results in a liberating stress on provisionality and diversity in a professional community of scholars whose strength is not the exclusion of other voices (of "contextualists" by "deconstructionists," or vice versa) but the awareness of how complex and open-ended the relationship between temporal selves and historical reality actually has been and remains. In this view, claims by advocates of intellectual "free play" are as totalizing as the claims of those critics who circle the wagons around the historical discipline by claiming "we" (a collectivity outside the flow of historical time unified in its understanding of historical knowledge?) will continue to do what "we" have always done. Neither position offers a fully

worked-out sense that, on the basis of Kracauer's comparison of photography and history, historians are engaged in the (itself historical, hence changing and composite) practice of producing "shared and sharable acts of . . . reading" shaped in particular ways "by different degrees of temporal distance and of acculturation in perception" (p. 201). For Kracauer, the historian, like the photographer, never works along a continuum simplistically marked out by "objective" and "subjective" poles, but rather along one anchored by "fact-oriented" and "impersonal" representations on the one side and by unstable representations whose power derives from their achieving a temporary equilibrium between spontaneity and receptivity to data on the other. Kracauer clearly valued this latter side of the continuum because it effectively and creatively exploited the full potential of the discipline, of photography as well as history.

Kracauer based his idea of historical (and photographic) method on the "historicity of understanding" (p. 247), to use Barnouw's phrase. This referred in part to the temporal and cultural distance between the historian as collector and the historian as interpreter of a series of "facts" based on recorded events in a past whose experiences and understandings are different than one's own. But it also was based on a keen appreciation of the fleetingness of the historian's world and the community of scholars to which he or she appeals. The "composite historicity of the life-world" (p. 250) of the historian makes all historical knowledge provisional and incomplete. Surely one cannot miss the importance of this observation for current epistemological debates. These debates, whether purely academic or increasingly public, are conducted along one line of reasoning as if the stakes are the final and definitive shape of historical knowing as such; as if just one approach to interpreting the past can emerge from the countless interactions that make up a discipline; as if, in short, historical knowledge and method have an atemporal quality that defines the very temporal subject matter it writes. Conversely, those historians such as Joyce Appleby (see "One Good Turn Deserves Another: Moving beyond the Linguistic; A Response to David Harlan," *AHR* 94 [December 1989]: 1326–32), whose position Barnouw respectfully criticizes, want to take seriously the postmodernist challenge to historical meaning by stressing methodological and political plurality, but they ignore the historicity of their practice, falling back into an atemporal perspective as unaware of the postmodernist challenge as their more conservative antagonists' position is. Trying to avoid this atemporality is not the same as saying historical interpretation is immediately transient and therefore open to an infinite array of readings; historical knowing is defined in part by its responsibility to other knowing subjects' making sense of a body of evidence on the basis of authoritative methods and narrative constructions. But the "anteroom" quality of such authorita-

tive methods and constructions removes all finality, all chance of completeness of "the narrative created by [the historian's] choices" (p. 252). These choices depend very much on time and place—Kracauer called the historian a time traveler who not only studied the past but also developed a temporal self—and to reconstruct the past now may be very different than reconstructing it in the future.

Significantly, Kracauer did allow for the possibility of what he called technical history, the ceaseless collection of a potentially complete set of facts. But his acceptance of this possibility was based on an approach unlike that of Walter Benjamin, who praised the collector of historical artifacts out of a conviction that a civilization would redeem itself and the entirety of its past in the fullness of time, on Judgment Day. This "theological" element was absent from Kracauer's critical realism, which stressed secular redemption and reconstruction of past time rather than total resolution. The ceaseless striving for facts, for a complete record of the past, was allowable for Kracauer only for reasons of "pity with the dead" (p. 256), which in any case did not presuppose final resolution, but only partitive stories within the historian's temporally bounded life-world. Just as Kracauer's photographer/historian should be equally wary of those preaching the timeless value of "great books" or those promoting the ceaseless evisceration of historical meaning, the historian should also be cautious about allowing his or her work to become the basis of "theological" projects of historical memory praising or deriding past events and identities, as the case may be. In this regard, I take Kracauer's critical realism to be a useful expression of the historian's necessary resistance to all forms of instrumentalization, and to all claims that what is considered true and valuable historically today will necessarily be considered true and valuable tomorrow.

I find Barnouw's discussion of Kracauer challenging and compelling, just as I find Kracauer's critical realism a useful antidote to the many outrageous claims made by historians, politicians, and pundits in contemporary, loaded political and epistemological debates. (Are they debates, or simply undignified shouting matches?) Barnouw's prose is at times infelicitous or overly complex, but the book is more than worth the intellectual investment. Historians familiar with Kracauer's history of German cinema will at times find Barnouw's Kracauer unrecognizable. *From Caligari to Hitler* was, after all, an impassioned indictment of how German film reflected and contributed to a culture's lockstep retrogression to totalitarianism. Barnouw refers to Caligari's selective and undifferentiated "thesis of a corresponding social-psychological and artistic collectivity" (p. 101), but generally she lets Kracauer off the hook too easily, arguing that unlike the Marxist Freudians of the Frankfurt school of the 1920s, Kracauer assumed German viewers had some dignity as cultural consumers. Readers of *Caligari* will, I think, find this an overly generous assess-

ment of Kracauer's position. Nonetheless, Barnouw is unerring in her judgment that Kracauer's broader thesis of a correspondence between the equally composite and unstable modes of historical and photographic representation has much to offer historians of the late twentieth century who are looking for a

perspective from which to engage the cacophonous cultural and academic debates that plague the contemporary scene.

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Reviews of Books

GENERAL

STEPHEN E. AMBROSE. *D-Day June 6, 1944: The Climactic Battle of World War II*. New York: Simon and Schuster. 1994. Pp. 655. \$30.00.

Stephen E. Ambrose's book on D-Day has scaled the heights: a selection of the Book-of-the-Month and History Book clubs, nine weeks on the best-seller list, the most heralded of the works commemorating the fiftieth anniversary of that fateful day. A well-known historian in his own right, Ambrose acknowledges his many debts in writing the book, from Forrest Pogue, the noted American military historian, who was actually interviewing wounded men offshore on June 6, 1944, to Cornelius Ryan, whose *The Longest Day* (1959) became a classic in the use of first-hand accounts to depict the Normandy assault. Ambrose's outstanding work continues that tradition, for at its heart are 1,380 oral histories that, as director, he and others at the Eisenhower Center in New Orleans undertook and gathered from other sources to describe the battle.

Ambrose's intent is to provide a popular, up-to-date version of the invasion and to have it serve as an inspiring reminder of what democracies, when roused, can accomplish. He succeeds on both counts, and even the most knowledgeable historian will gain new insights into the background and execution of the operation.

The book's most noteworthy feature is its gripping narrative. Ambrose writes exceedingly well, and his use of the oral accounts to illustrate the horror and valor of war makes for compelling reading. Ambrose's centerpiece is the American landings at Omaha Beach, to which he devotes nearly one-third of his 583-page narrative. One of the soldiers, Sergeant Harry Bare, described getting ashore as follows: "We waded to the sand and threw ourselves down and the men were frozen, unable to move. My radio man had his head blown off three yards from me. The beach was covered with 'bodies,' men with no legs, no arms—God it was awful" (p. 331). As for getting off the beaches in the face of enemy fire, Private Raymond Howell explained his thought process. He remembers thinking, "If I am going to die, to hell with it, I'm not going to die here. The next bunch of

guys that go over that . . . wall, I'm going with them. So I don't know who else, I guess all of us, decided, well it's time to start" (p. 345). Of course, others besides Howell also made it up the bluffs to the commanding heights above, so that "bloody" Omaha could be held.

Ambrose covers not only the combat side of the battle but also delves into seldom discussed aspects, such as telling vignettes about the reactions of individual French citizens in the Calvados landing area, the role of American women doing factory work, and the involvement of the 320th Barrage Balloon Battalion, one of the few African-American formations to take part in the invasion. He also describes the broadcasts of Axis Sally, the Ohio native but longtime Berlin resident who mesmerized numerous GIs and British soldiers with her "sweet, sexy voice," but who frightened them on occasion with her knowledge about specific allied units.

Ambrose also discusses technological features of the operation in understandable terms. For instance, his description of the German defenses from shore obstacles to reinforced concrete fortifications are graphic as well as accurate, and he further gives proper due to a number of British "inventions," including midget submarines that were to guide DDs ("swimming tanks") to shore and tanks with flails to detonate land mines. Neither does he neglect the often overlooked American B-26 medium bombers and P-47 fighters, whose pilots and crews played extremely important roles in the successful campaign to gain air superiority over the beachhead and beyond.

Moreover, Ambrose rightly emphasizes allied landing craft, that precious commodity, and its intrepid, civilian developer, Andrew Higgins. Not by chance did the U.S. Congress in 1992 authorize the building of the National D-Day Museum in New Orleans, on the site where Higgins and his employees built and tested his boats.

Besides Higgins, Ambrose fills his book with other heroes, from the lowest in rank to the highest. He is particularly impressed by the airborne and infantry troops, and, as one might expect from a biographer of Dwight Eisenhower, Ambrose accords the American supreme commander a prominent part in the

book. The author's attempt to compare Ike with Field Marshal Erwin Rommel, the German tactical commander, however, is overdrawn. It would have been more appropriate to compare Eisenhower with his German counterpart, Field Marshal Gerd von Rundstedt, and Rommel with either of the Allies' ground commanders, the American General Omar Bradley, or the British General Sir Bernard Montgomery. But neither Rundstedt nor Bradley adequately fits the heroic mold, and although the flamboyant Montgomery might have been appropriate, his abrasive egocentrism eliminated him as a possibility. Ambrose carries the Eisenhower-Rommel comparison, despite its being insightful and masterfully etched, too far.

The book also contains other disputable points. Among them is Ambrose's generalization that the Atlantic Wall was "one of the greatest blunders in military history" (p. 577), a statement that ignores the fact that Germany's holding of numerous harbors and their approaches helped cause an allied logistic crisis in the late summer and fall of 1944, which, in turn, helped prolong the European war into 1945. In addition, even though Ambrose dispels the myth that British and American troops did not train sufficiently for the assault, his contention that the Axis soldiers spent their time primarily building defensive barriers is wide of the mark. They both constructed and trained, and their performance on D-Day was not as deficient as alleged. Also, try as Ambrose might to be fair to each of the allied nations, the Americans emerge as the true heroes, and the other partners—the Canadians, the French, the Poles, the Dutch, even the British—at best become supporting members of the cast.

Nevertheless, although Ambrose's traditional approach will bother some historians, none will deny his ability to combine a first-rate narrative with a significant theme. He also does not gloss over what he considers allied military mistakes, such as the dropping of the U.S. airborne troops at night and the unreadiness of ground forces for hedgerow combat. But over and over his main point is that, although the allied soldiers, sailors, and airmen fought well, they would have much preferred not to have been fighting at all. In this sense, D-Day forms an appropriate link with the democratic tradition Ambrose extols.

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GERHARD L. WEINBERG. *A World at Arms: A Global History of World War II*. New York: Cambridge University Press. 1994. Pp. xix, 1178. \$34.95.

To produce a history of a global war must be tremendously satisfying to any historian. That Gerhard L. Weinberg has joined the club started by Thucydides in the fifth century B.C. is not at all surprising. For over thirty years he has been a prominent diplomatic historian, with such important works to his credit as *The Foreign Policy of Hitler's Germany* (1970), *World in*

Balance (1981), and *Germany and the Soviet Union* (1954). While directing the American Historical Association's project for microfilming captured German documents in the 1950s, he found and edited Adolf Hitler's "Second Book," an important testimony from the year 1928.

Weinberg's most recent opus is a breathtaking achievement in its sheer vastness. There are some inevitable shortcomings: the mere twenty-three maps are poor, and there is no chronological table to help the reader navigate this global epic saga. The only other work of comparable magnitude that comes to my mind is *Total War* by Peter Calvocoressi and Guy Wint (1972, rev. 1989), with more than 100 splendid maps and 160 pictures; but it keeps the global conflict rigidly separate in two theaters of war. Weinberg, however, has tried to integrate war activities in Europe and Asia into one picture. He touches on almost every aspect of global warfare: technologies of mechanized and electronic warfare, actions on land, in the air, above and under water, soldiers on the front lines as well as the home front and resistance movements, economics, and racial extermination policies, military intelligence, and so on. The over 900-page long narrative is further supported by a bibliographical essay and an array of amazingly detailed endnotes (some 200 pages), drawn from diplomatic and military documents. The notes are often more dramatic than the flat text. German primary sources, then British and American, predominate; the Russian and Japanese sides of the story derive mostly from secondary works.

Weinberg is an "intentionalist" *par excellence*. In one sentence he dismisses the "structuralists" and those contemplating the two world wars as one artificial unit within a thirty-year-long "European Civil War." In Weinberg's view the two world wars were fundamentally different, for Hitler, in contrast to the Wilhelmine imperialists of 1914, was preparing from the very outset global extermination of entire peoples considered subhuman, for the sake of a radical "racial reconstruction" of the world.

In spite of having the best intention of providing a global view of the entire war, reserving proportionately equal space to the non-European war theaters, Weinberg remains the type of distinguished Eurocentrist historian he has always been, for whom the *Dai Toa Senso*, the "Greater East Asia War," appears on the horizon as a distant war in a distant part of the globe, whose major components and whose rhythm itself remain sealed to him. Let us take, for instance, the case of Subhas Chandra Bose, whose name appears distinctly in Weinberg's preface as that of a chosen protagonist of the Eurasian dual continent at war. Did Bose really wish, as Weinberg tells us, that Germany and Japan would defeat Britain, the United States, and the Soviet Union? He certainly wished to see the British empire finished, but there is no evidence that he desired the same fate for the United States, even less for the Soviet Union, a country that

all the radical Asian nationalists considered an ally against British imperialism. Because I happened to publish an entire book on this subject (*India in Axis Strategy* [1981]), I am particularly concerned to correct certain widely held misconceptions on this issue. Bose indeed saw the outbreak of war in Europe as the golden opportunity for extracting India from the British Raj. Having been turned down by Moscow, he had no choice than to go to Berlin. Having been told by Hitler in May 1942 that Germany would not issue a Free India declaration, however, he decided to return to Asia and collaborate with the Japanese. Not being able to travel overland via the Soviet Union as he did in the spring of 1941, he had to delay his departure for Asia until early 1943 when arrangements could be made for two submarines, one German and the other Japanese, for the transfer of the Axis' most important revolutionary.

Bose's propaganda alone could not have achieved the liberation of India and the demise of the British empire. This could have succeeded only in conjunction with genuinely concerted Axis military operations. Fortunately (in my view) for the world, the two Axis powers failed completely, for the last time in the summer of 1942, to join their hands in a concerted military campaign across the Middle East and the Indian Ocean, and by exploiting the latent anti-Western feelings, harbored especially among the Muslim population in the immense segment stretching from Casablanca to Jakarta. Instead of calling on the vast pool of anti-Western forces, the Axis powers fought—and eventually lost—a series of separate campaigns in two isolated theaters of war. For it was in those critical weeks after Pearl Harbor that the two coalitions moved in two diametrically opposite directions. In spite of Subhas Chandra Bose pointing to the Indian subcontinent, where the most significant anti-Western uprising of World War II was about to start ("Quit India" in August 1942), each Axis partner pursued selfishly its own strategy. By contrast, the British and Americans came together, physically and in spirit, and thus could open a truly global strategy. Once the tide of the great global war had moved from one coalition to the other, the rest of the saga dealing with the "world at arms" seems fairly straightforward.

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JOYCE APPLEBY, MARGARET JACOB, and LYNN HUNT.
Telling the Truth about History. New York: W. W. Norton. 1994. Pp. 322. \$25.00.

The title of this book by Joyce Appleby, Margaret Jacob, and Lynn Hunt seems to require a sustained philosophical analysis of its gerund and two nouns, yet most of it, and the best of it, is devoted to an account of the history of historiography—much, indeed, to the history of science—intended to show why the notion of "the" truth about history has become

problematic. So readers looking to it for a treatise on the theory of history will probably be disappointed; yet anyone following the leisurely development of its argument will be instructed and perhaps reassured. I use "anyone" advisedly; its prose is clear, its tone genial, and its forays into philosophy sufficiently unthreatening for intelligent undergraduates, while professional historians can profit from this reflection on what their discipline can accomplish.

The first third of the book traces the rise of the idea of scientific history and the way that a national historical myth was elaborated in nineteenth-century America. It places great—excessive, I believe—emphasis on the influence of Newtonian science, which certainly underlay the positivist conception of history, although it had little effect on actual historical practice. The book then describes the collapse of the positivist paradigm, attributed to the discrediting of the idea of value-free science by the ease with which science has been put to antisocial uses; the revelations by "externalist" historians of science that scientific research is far less pure and unworldly than the way any philosophers of science had represented it; and the democratization of the American academy as affirmative action (and the GI Bill before it) opened its doors to a far wider sample of the population than ever got university education prior to World War II. This hopelessly complicated the hegemonic story of American virtue, liberty, and God-granted territorial expansion by inserting the experiences of ordinary immigrants, women, and African Americans. (The fact that the authors present themselves as beneficiaries of this development gives the book a particular poignancy, since many feminists have come to regard "truth" and "objectivity" as masculinist constructions.)

Adding to the perplexity that (in their view) besets contemporary American historians is the loosening of ideologies that has come with the end of the Cold War, removing the landmarks set by the confrontation of the United States and the Soviet Union. Finally, postmodern or poststructuralist theories of language, such as the claims by Jacques Derrida and Michel Foucault that language has only an irredeemably slippery hold on reality, undermined confidence that historians can give a verbal copy of "what really happened." Although few American historians can have read these difficult and often paradoxical thinkers, they revived the skeptical challenge first launched in the 1930s by Charles Beard and Carl Becker.

Although our present discontents in historiography are undoubtedly overdetermined, there is room for questioning the authors' tracing of the causes. Their history of historiography omits the entire tradition stressing the autonomy of history as against the methods and claims of natural science. "Externalist" histories of science probably have had limited impact on most historians, and their influence is overstated; the "internalists," as the career of Alexandre Koyré and his disciples illustrates, were at least equally

influential, and raised fewer disturbing questions about the scientific enterprise.

Whatever the causes, however, the notion of historical truth has come under attack. How can it be defended? The authors sketch out a position they call "practical realism," which has all the commonsensical appeal of the middle ground. It is distinguishable from naive realism because it renounces any hope to reach final, objective historical truth, which nevertheless remains a goal at which to aim. But they claim it escapes the perils of relativism by holding that truth—a closer and closer approach to truth, at least—is attainable through disciplined historical inquiry in a culture freely allowing criticism and controversy. Historians' truth claims are always provisional and subject to revision or refutation by well-established criteria, but can be "reasonably, if partially, true" (p. 229).

The practical side of this position seems sensible, if a bit optimistic, given the consensuses that historians have managed to reach in the past. The authors defend a historical practice conducive to "a more intellectually alive, democratic community" (p. 229). It is not entirely surprising that after avoiding utilitarian arguments for objectivity, the authors eventually try to combine pragmatism with "practical realism." Pragmatism, I think, would offer the authors all that they need, and, in any case, all they can have. It is not, however, all that they want. They seem to believe that practice has to be grounded somewhere; hence their invocation of realism and talk of objectivity and "the truth." It is true that pragmatism may seem a weak reed. The bedrock pragmatist position, that truth is what works, poses the question: "Works for whom?" These authors, or Lynne Cheney?

The book, however, skates too lightly over the difficulties posed by even "practical" realism, especially when forced into a marriage with pragmatism. Granting that historians "only have the traces or residues of the past" (p. 234), that they make choices that are aesthetic, political, and social as well as epistemological, and that "there can be a multiplicity of accurate histories" (p. 252), they produce an extremely chastened version of realism. But to claim that "personal interpretive assumptions guide historians as they compose their stories," and yet that these are "objective" (p. 263), is to cling to a foundationalism that the pragmatists rejected. So is their reference to telescopes, "objects out there," and advice "not to divert the lens from the object—as the relativist suggests—but to leave it on and keep trying to clean it" (p. 269). No analogy holds between the past—the "object" of history—and visible objects. The authors appear to believe that foundationalism of this—admittedly modest—sort is the only way to avoid the endless play of signifiers, total relativism, and solipsism. But these positions are either self-defeating or easily defeated on pragmatist grounds. "The relativist" to be dealt with is not one who embraces a daft policy of not considering any evi-

dence, but one who believes that the evidence underdetermines the stories that can be told about it—their "multiplicity of accurate histories," in fact. Calling even this "objectivity" or "telling the truth about history" may make many of us feel better, but it risks giving a reassurance that has not been fully earned.

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PETER GAY. *The Bourgeois Experience, Victoria to Freud*. Volume 3, *The Cultivation of Hatred*. New York: W. W. Norton. 1993. Pp. x, 685. \$30.00.

This is the third volume of Peter Gay's pathbreaking study of the middle classes in Europe and the United States in the century preceding World War I. The first, *Education of the Senses* (1984), in its nuanced sensitivity to the give and take between culture and experience, surpassed all previous efforts to recover emotional dimensions of the past. The second, *The Tender Passion* (1986), undermined stereotypes of Victorians' repression by exploring how they blended love and sexuality. This new volume differs from the first in attending less to experience than to intellectual controversy. It is less clearly focused than *Tender Passion*; although it is erudite and fascinating, it is harder to say what exactly it is about. Its ostensible subject, hatred, often disappears, while Gay probes shifting bourgeois attitudes toward aggression.

The notion of "cultivating" hatred conveys a double meaning. Individuals "get pleasure from the exercise of their aggressive powers" (p. 9), while societies must limit the accepted range of aggressive behavior. Gay opens with the gore of German student dueling: the scars its veterans bore with pride and regulations put forward to control violence epitomize the two sides of the paradox of cultivation. The first of six long chapters, each an important essay in its own right, examines intellectual movements—Social Darwinism, racism, imperialism, and theories of manliness—that exonerated and set boundaries for aggressive acts ranging from wanton cruelty to civilized domination. The second chapter reveals recurrent efforts by intellectuals to celebrate their era's progress toward civility, by analysts to isolate the causes of pathologies like sadism and suicide, and by reformers to extirpate atavistic punishments and brutal behavior from social institutions. A third chapter traces the invention of politics: abandonment of belief that the state's opponents must be eliminated, acceptance of codes of legitimate dissent, and "the advent of democracy, that supreme end toward which the redefinition of public aggression was steering" (p. 219). A fourth chapter returns to prominent themes of earlier volumes: female sexuality and the separation of private and public spheres. It emphasizes endeavors to reserve aggression as male privilege and ambivalent and contradictory images of women's natural rapacity and weakness and of their domesticated power and submissiveness. In this cultural con-

finement women advanced toward literary and political influence by denying aggressive intentions. The fifth chapter analyzes forms of humor blending gaiety with darker moods and argues that bourgeois, having tamed aggressive fantasies in many ways, found outlets in jokes directed against others—and sometimes against themselves. Some of their laughter was restrained; some lapsed closer to sheer cruelty. In the final chapter, the usefulness of disciplined aggression is shown in justifications of a wide range of activities, from sports to philanthropy, as substitutes for warlike behavior and, especially, in the progress of specialized, professionalized work in natural and social science. But this was also an “Age of Neuroses” (p. 506), a century of hysteria and obsession, in which a mountainous body of advice literature depicted conflict between biological drives and human capacities for cultivation and self-control. An epilogue surveys hatred’s resurgence in 1914.

A brief review cannot do justice to the illuminating insights on countless subjects that result from Gay’s transatlantic perspective. No doubt specialists will offer corrections and additions for the narrower realms they have mastered, but Gay’s achievement is formidable. Thinking through these volumes’ provocative claims will occupy scholars for decades. This volume may lack the tight schematic focus of Gay’s earlier work, but its purposes will be clearer if readers look first at the appendix on “Theories of Aggression.” Gay does not study hatred as one of several primary affects; he moves from one topic to another on the Freudian assumption of a single human drive connecting many pursuits of mastery. But he is not simply applying psychoanalytic doctrines. Impressed by post-Freudian recognition that aggression’s sources and consequences are “protean,” he rejects “premature and simplistic verdicts” on human endeavor in the past. It is the “inconclusiveness” (p. 536) of theory, not its certainty, that opens the way for this brilliant work of historical investigation.

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NANCY TUANA. *The Less Noble Sex: Scientific, Religious, and Philosophical Conceptions of Woman’s Nature*. (Race, Gender, and Science.) Bloomington: Indiana University Press. 1993. Pp. xii, 224. Cloth \$29.95, paper \$12.95.

Nancy Tuana’s book is brilliant. In under two hundred pages she presents a concise account of how women have been perceived in relation to men in the Western world for the past 2,500 years. For whatever reasons, our ancestors separated the sexes with regard to their relative value with a surprising number of similarities among the disparate traditions. The females’ inferiority to males in our species places them close to children and creatures of other species. Tuana documents her case with an abundance of fascinating detail.

The book begins with an account of the ancient Greeks’ descriptions of woman’s nature. Tuana draws our attention to Aristotle’s explanation of woman’s “imperfection”: the female is seen as “a mutilated male,” and he described women as “monstrosities.” These ideas persisted into the nineteenth century. Tuana points out that, in spite of huge advances in biology in the nineteenth century, “sciences’ view of women as between man and animal remained fundamentally unchanged.” “Though these days ‘different and complementary’ are substituted,” she continues, “difference was, as it is still, seen through the lens of a hierarchical system” (p. 50).

Tuana ends with the hope that the clichés regarding general conceptions of the sexes will be rejected, that the age-old dichotomies will soon be replaced so as to “expand our notion of difference and work to create new ways of speaking, a new metaphysics” (p. 172).

This book is an eloquent appeal in the struggle to achieve equality for both sexes. Tuana does not indulge in polemics, and her writing is clear and trenchant. Her study deserves to be read widely.

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PETER N. STEARNS. *The Industrial Revolution in World History*. (Essays in World History.) Boulder, Colo.: Westview. 1993. Pp. xiii, 254. Cloth \$54.95, paper \$12.95.

Historians will have several reasons to welcome Peter N. Stearns’s new history of industrialization. It is a brief and straightforward account, suitable for use in undergraduate courses. More importantly, it portrays industrialization as an ongoing global process, thereby combining three genres normally found in three different parts of the historical literature: the Industrial Revolution in Great Britain; the industrialization of Europe, the United States, and Japan; and the “development” or “underdevelopment” of the Third World. Stearns’s goal of presenting this vast and complex topic in 234 pages is certainly ambitious and his book deserves to be read widely.

Stearns divides industrialization into three phases. The first phase runs from 1760 to 1880 and includes not only the industrial revolutions in Britain, Western Europe, and the United States but also abortive attempts to industrialize in places such as India, Egypt, and Latin America. The second phase, which runs from 1880 to 1950, introduces new developments in the pioneering countries, the industrialization of Japan and Russia, and the impact of those developments on the global economy, including the “new” imperialism in Africa. The third phase, from 1950 to the 1990s, addresses recent and still poorly understood issues such as the industrialization of the Pacific Rim, the unfinished industrialization of Brazil,

India, and China, and the industrial implosion of the former Soviet Union.

Overall, Stearns sees industrialization as a widening process, of irregular and unpredictable shape. Although he criticizes both the modernization and dependency models, he does not offer an alternative model but prefers to stress a variety of causes: American aid to South Korea and Taiwan, immigrants to Israel, import-substitution policies in India and Brazil, and liberalization in China, to name a few. He mentions culture and education, but only briefly. Unlike Paul Kennedy and other postindustrial prophets, Stearns does not attempt to outline future scenarios or predict the next "rise" and "fall;" no doubt, after the events of 1989, prediction itself has become too risky. Instead, he ends his book with a sense of wonder and despair at the diversity of human experiences.

Readers of Stearns's previous books will not be surprised that the great strength of this work is its description of the social consequences of industrialization, especially for working men and women, children, migrants, and city dwellers. The classic story of factory work, workers' lives, and labor movements in the nineteenth century is especially well told.

Although it is strong on social issues, this book represents the sort of economic history that economists deplore. Economic concepts are rare and economic analysis is almost nonexistent. The few statistics generally consist of isolated facts that cannot be used for comparative purposes. For example, Stearns claims that "Average annual strike rates in the United States during the 1960s were down approximately 15 percent from their 1950s rate" (p. 199). Instead, the author relies heavily on qualitative substitutes for statistics, such as "low-wage labor," "rapid growth," and "expanding numbers."

Historians of technology will be disappointed by Stearns's treatment of their specialty. Although he pays lip service to "technology" as one of the key factors in industrialization, his explanations of the basic machines and processes are vague, skimpy, or downright misleading. For example, Stearns writes that "New scientific knowledge about the behavior of gases set a context for considering the possibility of harnessing steam to provide a moving force to replace unreliable water and wind as power sources. The first steam engine was invented by a French refugee in Holland in the late 1600s . . . Around 1700 the engine was improved in England by Thomas Newcomen" (p. 19). Anyone not already familiar with the development of the steam engine will end up more confused than enlightened. Other crucial technologies—railroads, electricity, the internal-combustion engine—get even less explanation; instead, they appear out of nowhere and magically transform the world.

The environment also gets short shrift. Social historians all too often pay attention to the environment only when it affects humans in a negative way. In this book, the environment is strictly equated with air and

water pollution, toxic wastes, and nuclear threats. The author ignores all the industrial methods humans have used to transform the Earth for their own benefit, including dams and irrigation, agricultural machinery and chemicals, railroads and highways, beaches, suburbs, and parks. He also ignores the impact of environmental changes on the other creatures that share this planet with us.

This book, then, is a valiant attempt to cover a vast topic in an efficient way and will be used in many undergraduate courses in modern world history. Its limitations, however, will require other books to complement it.

DANIEL R. HEADRICK
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PAUL BAIROCH. *Economics and World History: Myths and Paradoxes*. Chicago: University of Chicago Press. 1993. Pp. xvi, 184.

This collection of fourteen essays is vintage Paul Bairoch: iconoclastic, imaginative in its juxtaposition of statistics, and, unfortunately, a bit sloppy in its scholarship. Bairoch tries to demolish a series of "myths" about world economic history, misconceptions about both the developed world and the so-called "Third World" or less-developed economies. More often than not, however, Bairoch fails to say who has asserted the "myths" he claims to deflate and jumps from case to case with little systematic thinking.

Bairoch is at his best dealing with the relations between the First and Third World. In contrast to underdevelopment theorists, Bairoch argues that the industrialization of Europe and North America did not depend heavily on less-developed economies for raw materials or markets. Too many scholars have generalized from the case of British textiles, which depended on importing raw cotton and exporting goods overseas. Neither, Bairoch argues, does exporting agricultural products necessarily doom tropical countries to underdevelopment. Many developed countries, he reminds us, exported such products while industrializing. The unprecedented recent growth in agricultural productivity in the First World, which Bairoch does a good job describing as an "unnoticed historical turning point," has radically weakened the position of grain producers in the Third World.

On the issue of protectionism and free trade, however, Bairoch appears confused. Most historians, at least since Hans Rosenberg, have argued that rising protectionism was one of the central trends of the late nineteenth century. Nonetheless, Bairoch claims to attack the myth of free trade in the years 1875 to 1914. In fact, his data suggest that the rise in tariffs in this period was modest. Excluding free-trade Britain and unusually protectionist Spain and Russia, table 3.3 shows that, on average, tariffs on manufactures in continental Europe rose from 9.4 percent to 13.9 percent. Not until the 1960s were tariffs this low

again. Similarly, I do not know whom Bairoch is attacking when he counters the "myth" that protectionism caused the Great Depression. For the world as a whole, over the long run, most economists argue that protectionism is not a good way to fight downturns, but this is not the same as saying that it causes recessions. Bairoch too often treats free trade and protectionism as isolated factors. Thus, if the recession of the 1870s occurred when free trade policies were in effect, this indicts free trade. If, after protectionism was adopted, economic growth improved, then "protectionist measures resulted in a distinct acceleration in economic growth" (p. 50). He does not consider that supply and demand, monetary policy, or productivity could have greater effects than trade policy. Business cycles, which so many scholars have worked on and which Bairoch ignores, could account for much of the change that he attributes to tariffs. Bairoch's query, "has protectionism always had a negative impact?", also seems off base. Many scholars recognize that individual countries have done well behind tariffs, or at least have not been dramatically hurt by them. What effect trade policy will have in any one country depends enormously on the policies that other countries adopt.

Bairoch's data often have to be read carefully. He argues that protectionist countries did better in the late nineteenth century than semi-protectionist ones. A close look at table 4.2 shows that Germany is in the "protectionist" category, although Table 3.3 shows that its tariffs were lower than those of Denmark, which somehow ends up as "semi-protectionist." Because Bairoch deals with only seven countries here and the difference in the average tariff between his two groups appears to have been only about 5 percent, the reader is understandably skeptical. In arguing that the Great Depression was not unusually severe, he maintains that eight European countries actually had better economic growth in the 1930s than in the 1920s. What Bairoch fails to emphasize is that the immediate post-World War I recession in most of these countries was very severe, something that almost everyone has recognized. If we compare the period 1925–29 to the 1930s, a different picture emerges. Table 1.2 shows that only Finland and Germany had higher growth rates in the 1930s than in the late 1920s or pre-1914 era. Excluding the Soviet Union, the other ten countries in Bairoch's table had a growth rate during 1890–1913 of 1.6 percent, during the late 1920s of 3.0 percent, and during the 1930s of 0.6 percent, apparently the lowest for any decade in over a century. Five of the ten countries had negative growth rates. Perhaps the Great Depression is not a "myth"?

Since Bairoch has demonstrated in his earlier work that he has real strengths as an energetic, wide-ranging scholar, one wishes he had chosen his "myths" more thoughtfully, covered the literature on them seriously, and used his data with more care. Instead, this is a provocative work with some occa-

sional insights and a good deal of unevenness as a piece of scholarship.

CARL STRIKWERDA
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JORDAN GOODMAN. *Tobacco in History: The Cultures of Dependence*. New York: Routledge. 1993. Pp. xi, 280. \$59.95.

Tobacco played a critical role in the emergence of the early modern world. An indigenous American product, it quickly became ubiquitous in the aftermath of the European invasion. Only a century after Christopher Columbus's initial voyage, "tobacco was either grown or consumed in most of the known world" (p. 37). As a consequence, Jordan Goodman's welcome survey of tobacco in world history takes on a huge and complex task. We should thus be willing to forgive him if the theme he has chosen to hold that topic together, neatly summarized in the book's subtitle, "the cultures of dependence," occasionally seems slightly strained.

Goodman suggests that growers who relied on income from the crop and governments that captured the tax revenues it generated became dependent in ways analogous to the nicotine-addicted user. That is a conceit more likely to obscure than to illuminate, because it implies that the decisions of growers and officials cannot be understood as those of rational actors operating within shifting constraints. The conceit, even if it is somewhat forced, does at least give a unity to Goodman's vast topic. The metaphor of dependence is most persuasive in the realm of consumption, which is Goodman's central interest. In some ways that is unfortunate, for tobacco in the early modern world was produced under vastly different circumstances: in some cases by slaves, elsewhere by free peasants, some of whom were "strong farmers," others poor tenants, barely hanging on. One could learn much about the varieties of labor organization in the early modern world by exploring how tobacco was produced in these various circumstances. That is not the book Goodman chose to write. If someone does decide to write such a book, his or her work will be made much easier by Goodman's impressive survey of a vast literature.

RUSSELL R. MENARD
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WALTER A. McDOUGALL. *Let the Sea Make a Noise . . . : History of the North Pacific from Magellan to MacArthur*. New York: BasicBooks. 1993. Pp. xii, 793. \$30.00.

This book by Walter A. McDougall landed on my desk wrapped in a handsome dust jacket bearing glowing endorsements from three Harvard professors, two leading journalist interpreters of Japan, and a renowned military historian. Their words—"a stimulating and original interpretation"; an "unusual, but

highly imaginative approach"; and "full of surprises"—made me eager to plunge into the massive tome. But before fifty pages had passed before my eyes, disappointment welled up within me. The gap between promise and product was so great that this wizened reviewer knew it was abnormal. I wondered, how could a Pulitzer Prize-winning historian have written *this*? *This* was random thoughts confided to his laptop computer while on a flight to Hawaii, an imagined dialogue with King Kamehameha's favorite consort, and a smattering of Spanish, Japanese, Chinese, and Russian history to 1689. An ominous beginning.

But I read on, lured by McDougall's style. It quickly became apparent that he is a gifted writer and master storyteller. I could see, too, that he had made the most of a year's fellowship at the rich Asian collection of the Hoover Institution and knows well how to surf the Internet and use good gophers so as to find all the best sources. What he has written is well grounded in a prodigious array of secondary English-language works on Russian, Japanese, Spanish, Chinese, and American history. McDougall extracted such witty quotes and pithy anecdotes from these materials that I knew instantly that no undergraduate could have dozed through his classes at Berkeley and the University of Pennsylvania. The author of this book, clearly, was a master synthesizer.

The more I read, however, the more obvious the gap between intent and result became. McDougall appeared to be trying to mix and surpass in the North Pacific what Fernand Braudel did for the Mediterranean, A. J. P. Taylor did for continental Europe, and Paul Kennedy did for the rise and fall of empires. He has imposed a grand design on his material. The book, like Caesar's Gaul, is divided into three parts, each of which consists of detailed chapters labeled by time and place. The first tells of Spanish, Russian, and American struggles in the age of sail and muscle power, to explore and find fortunes in the vast North Pacific region down to the mid-nineteenth century. The second part chronicles their imperialistic forays and conflicts in once-remote locales brought close by steam-powered ships and railroads. The third tells a familiar story with a different twist: how racial antagonisms within Japan and America and the development of the internal combustion engine for aircraft led to the Pacific War and, beyond it, to the Cold War. Some fourteen imagined dialogues with historical figures prominent in the story break up McDougall's narrative.

By the time I approached its seven-hundredth page, my sense of unease about what I had read crystallized into convictions as to what was wrong. McDougall is telling too many stories, in too much detail. Like Elizabeth Taylor who, in the old joke, never met a man she didn't marry, he just cannot leave a juicy anecdote out of the text. Then there are those dialogues. A technique appropriate for fiction falls flat on its face as history. Why? In part because

McDougall puts into the mouths of historical characters words that were cuter and more pretentious than anything they might have spoken or thought. More important, these conversations really do not provide the glue that might have brought intellectual coherence out of the massive detail that brackets them.

Finally, although there are insights and opinions aplenty in these pages, McDougall has not fashioned an interpretation from them. He does not explain why his story begins and ends where it does. A reader entering these pages wondering whether the forces that drove people on all sides of the Pacific to exploration, imperialism, and war were similar or different would leave them without an answer. McDougall's story clearly poses the question of whether violence was—and is—the inevitable by-product of racial and cultural differences. But I left the text not knowing his answer to that puzzle. Indeed, apart from tinges of Chicago school free-marketism and bits of *fin-de-siècle* racialism common among Californians alarmed by recent flood tides of Asian and Latin American immigration, I found few consistent ideas binding this text together.

Who, then, is likely to listen to the noises arising from McDougall's sea? Based as it is on their own works, specialists in American and East Asian history may well turn a deaf ear to the book. Undergraduates seeing its vastness may well turn back at its shores; their complaints at having to navigate so vast a sea with so little guidance will probably close their ears to whatever wisdom may lurk in its depths. Perhaps this volume is simply a beach-goer's book: something to take along, dip into now and again, and read for entertainment and occasional insight, but not for serious scholarly purpose.

ROGER DINGMAN
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SVEN STEINMO. *Taxation and Democracy: Swedish, British and American Approaches to Financing the Modern State*. New Haven: Yale University Press. 1993. Pp. xvii, 280. \$32.50.

Sven Steinmo argues that it is not the fundamental social values of a country that determine its form and level of taxation, and it is not a matter of contending special interests. Instead the decisive factor is the structure of political institutions and decision-making mechanisms. The United States has a "fragmented" structure, Sweden a "corporatist" structure, and Britain a parliamentary structure, and each system provides the government with near dictatorial but temporary power. He first outlines what he regards as widely divergent mixes of taxes in the three countries on which he concentrates (plus others in the Organization for Economic Cooperation and Development [OECD]). He professes to be surprised at finding that the United States has a generally progressive system, while the European countries rely heavily

on regressive measures like the value added tax (VAT).

Voters, says Steinmo, are generally poorly informed, but rationally self-interested. They want contradictory policies from their governments: expanded social services and lower taxes. In the United Kingdom and Sweden, policy—not economic—elites are able to institute programs of social services, even if that means raising what he calls “hidden” taxes like the VAT. In the United States, the fragmented system prevents policy elites from having that power, so that the “no new taxes” cry predominates, and the system cannot produce adequate social services. He wishes the United States had a more thorough welfare system and says that America may have “the most democratic political system in the modern world. This may be its undoing” (p. 209).

He thus reverses a picture of the modern period that sees “the people” demanding equality and elites resisting those demands. Instead, “the people” insist taxes must not rise, and elites, if they have the power, construct a system of increasing egalitarianism.

It is good to have taxation policy brought into the general historical picture and not confined to specialists. Steinmo’s arguments are provocative. But the evidence he supplies, as well as some he neglects, can be read in precisely the opposite direction from his thesis. Political structures may have some bearing on some details of taxation and welfare policies, but whatever the political structures, it is the values of a society, expressed through a variety of popular pressures, that determine broad policy outcomes.

The central portions of the book are brief histories of taxation or taxation/welfare policies in the three countries. In the section on the United States he says that “equity considerations” shape America’s income tax and that opposition to regressive taxes prevented the institution of a VAT. What are these if not value judgments? Yes, the power of local government may have meant that state and municipal bonds are tax exempt, but his description of the general structure of the U.S. tax system has nothing to do with a “fragmented” polity.

In claiming extraordinary power for the British government, Steinmo neglects such important popular movements toward an extension of the welfare state as the National Pension Committee, the Trades Union Congress, and delegations from the Labor and Liberal parties to Germany. One gets the picture of policy elites developing the taxation and welfare system quite apart from the voters, but that is not the way it happened. The Lib-Lab coalition was responding to pressures.

Steinmo may have a stronger case for Sweden. But the permanent civil service, the broad interest groups of employers and workers, and policy experts outside of government have the same point of view not because of structures but because of an extraordinary consensus—values—within Swedish society. Even when a “bourgeois” coalition is elected, very little

about the Swedish welfare state changes. He claims that “there is no evidence” that Swedes like taxes any better than Americans. There is in fact overwhelming evidence: Swedes are willing to pay at almost twice the level of Americans. Steinmo talks about “hidden” taxes in Sweden, like the VAT. Hidden from whom? Do Swedes not know they are paying high taxes?

Steinmo argues that structures are determinative. I would argue that values are. As I read his evidence, I find that he in fact supports my position.

DANIEL LEVINE
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IAN S. LUSTICK. *Unsettled States, Disputed Lands: Britain and Ireland, France and Algeria, Israel and the West Bank–Gaza*. (The Wilder House Series in Politics, History, and Culture.) Ithaca: Cornell University Press. 1993. Pp. xiv, 576. \$37.50.

In the 1990s the impermanence of national boundaries is a clear fact of life in substantial portions of Eastern Europe, the Middle East, Asia, and Africa. Ian S. Lustick seeks to provide a theoretical framework for understanding the dynamics of state expansion and state contraction. There can be no question, therefore, about the timeliness of this book. It is somewhat curious, however, that the author makes no reference to Paul Kennedy’s *Rise and Fall of the Great Powers* (1987), which considered similar themes and attracted considerable attention less than a decade ago.

Lustick examines two historical instances of state contraction: Britain’s relationship to Ireland from the 1830s to 1922 and France’s relationship with Algeria from the 1930s to 1962. He then applies the lessons derived from these examples to current territorial questions facing the state of Israel with regard to occupied territories in the Gaza Strip and the West Bank of the Jordan River.

Lustick’s discussion of the history of the Irish and Algerian questions is relatively straightforward and based on a close reading of the standard secondary literature. These chapters are interspersed with lengthy discussions introducing and applying an analytical framework that posits a three-stage, two-threshold paradigm of territorial state building and state contraction. A short appendix provides diagrams of the model. When in an expanding mode, a given state moves from the “regime stage” across the “ideological hegemony threshold,” where there exists a genuine national consensus about the maintenance of occupied territories (for example, Britain/Ireland, 1830s–80s). Disengagement at the stage of ideological hegemony would require the occupied territory to secede from the mother country. When in a contracting mode, however, a state moves in the opposite direction from the “regime stage” across the “regime threshold” to the “incumbency stage.” Whereas the relinquishment of an occupied territory in the “regime stage” would very likely threaten the legal/

parliamentary order, in the "incumbency stage" it would do no more than pose threats to the political careers of particular incumbent leaders or parties (for example, France/Algeria, 1958–62). Disengagement at the latter point would ordinarily take the form of decolonization rather than secession.

The author, a specialist in Middle Eastern affairs, then extends and tests his theory by describing the conditions that would be necessary for Israel either to absorb or relinquish the West Bank and Gaza Strip. Lustick believes that, since the inception of the *intifada* in 1987, Israel has moved slowly in the direction of state contraction, indicated most clearly by the electoral defeat of the Likud Party. A final chapter posits ways in which the model can be used to assess the character of new states emerging from the breakup of the Soviet Union and the transformation of multiethnic societies in other parts of the world.

Some will find this book a useful theoretical contribution in spite of its occasionally dense narrative thickets. This sentence, from one of the theoretical chapters, is an example: "These facets of institutionalization and de-institutionalization processes can be located in relation to one another if the continuous aspects of institution-building, including gradually increasing propensities to expect norms, rules, and boundaries to be adhered to and symbols to be honored, are understood to surround two distinct thresholds" (p. 37). Others, however, may question the need for an elaborate theoretical structure to explain that states are likely, through force majeure, to acquire and maintain alien territories as long as a national consensus supports expansion. Conversely, states are more likely to relinquish occupied territories as the national consensus evaporates.

Regardless of one's assessment of the theoretical apparatus, Lustick's book will be of interest to scholars in the fields of international relations, modern British and French history, and Middle East studies.

WILLIAM I. SHORROCK
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RODERICK A. McDONALD. *The Economy and Material Culture of Slaves: Goods and Chattels on the Sugar Plantations of Jamaica and Louisiana*. Baton Rouge: Louisiana State University Press. 1993. Pp. xiv, 339. \$39.95.

Whereas historians of the Caribbean have long understood the slaves' role in local provision economies, students of North America have only recently begun to investigate this aspect of the slave experience. Assuming that "mature sugar plantation societies" (p. 1) embodied certain common characteristics, Roderick A. McDonald focuses his attention on slaves' independent production and exchange in Jamaica and Louisiana. His study establishes important methodological and conceptual reference points for further investigation.

The comparative framework enables McDonald to examine similar phenomena in the two regions with

telling effect. In Jamaica, for instance, he indicates that by 1792 the law required planters to allocate both land and time for slaves to provision themselves. Louisiana law imposed no such mandate, but slaves there nonetheless reached understandings with their masters allowing certain rights. These included access to kitchen gardens for raising vegetables, to provision grounds for raising corn, and to markets for disposing of their goods. McDonald successfully resists the temptation to romanticize, a major pitfall to this kind of investigation. Instead, he notes the heavy burden on the slaves that such arrangements entailed. At the same time, he wishes to understand the various material and social benefits that slaves might have derived from their independent production and exchange.

McDonald argues pointedly that slaves produced to consume. In Jamaica they largely clothed, sheltered, and fed themselves, with their household compounds serving as the centers of family based economic activity. In so doing they profoundly influenced the material culture of the island. Moreover, possessing goods gave rise to an ideology of proprietorship wherein slaves considered themselves "property holders, with the right to keep, protect, and bequeath their possessions and territory" (p. 110). This ideology pervaded Jamaican slavery and provided an essential point of departure for emancipation.

Although Louisiana slaves did not exercise comparable control over land and dwellings, they employed their earnings to supplement rations of food and clothing, purchase tobacco and alcohol, and otherwise improve their material lives. McDonald takes pains to point out that not all slaves enjoyed the privilege of producing and exchanging and that, even for those who did, other privations and burdens of bondage remained, at times in intensified form. The slaves' internal economy, in short, was not an un-mixed blessing: ostensibly beneficial features often disguised opposite tendencies.

Given this book's manifest virtues, one weakness stands out. Nearly one-third of the book consists of appendixes that illustrate from primary sources various aspects of slave material life in Jamaica and Louisiana. Although these tables are fascinating, they do not appear to warrant the space they occupy. Given the comparative nature and sweeping chronology of his work, readers might ask for more analysis and less documentation.

In particular, McDonald seems to slight the historical development of slaves' internal economies. Focusing on a plantation society's "mature" phase begs the questions of what constitutes maturity and how a society achieves that state. Although his analysis of Jamaica accounts for change over time and the give and take among slaves, masters, and colonial authorities, his treatment of Louisiana appears less dynamic in comparison. Given the rich literature on slaves' independent production and exchange in the rice plantation region of South Carolina and Georgia,

McDonald passed up a marvelous opportunity to offer a comparable chronology for Louisiana's sugar country. Finally, the keen insight and balanced judgment that he displays throughout creates a yearning for additional commentary on the relationship between the slaves' internal economy and their larger political struggles.

These criticisms notwithstanding, McDonald's book makes a major contribution to the historiography of nineteenth-century slavery. By focusing so sharply on the "goods and chattels" that derived from slaves' production, consumption, and exchange, he challenges historians to reconceptualize the material foundation of slave life and its broader ramifications. Future assessments of the peculiar institution cannot but benefit from McDonald's important work.

JOSEPH P. REIDY
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STEPHEN VELICHENKO. *Shaping Identity in Eastern Europe and Russia: Soviet-Russian and Polish Accounts of Ukrainian History, 1914–1991*. New York: St. Martin's. 1993. Pp. 266. \$35.00.

The history of Ukraine, a region contested by Poland and Russia for 300 years, remained a sensitive topic for Polish and Soviet historians in the twentieth century. Stephen Velychenko's book analyzes this political sensitivity by tracing the evolution of interpretations of Ukraine's past in survey histories and monographs published in Poland, the Soviet Union, and Soviet Ukraine. The author demonstrates how these surveys reflected Communist Party directives on historiography and how scholars implemented the party's ideological manicheism. But, as Velychenko admits, the interpretive guidelines were not uniformly fulfilled. Historians in both countries managed to express differing interpretations, especially after Joseph Stalin's death.

The author navigates Polish and Soviet historiography with great skill. He not only summarizes the outlines of trends within Soviet, Soviet Ukrainian, and Polish historiography but also carefully analyzes the internal contradictions of these histories.

Polish treatment of Ukraine's past, especially after 1948, provided a more dispassionate and balanced view than the Soviet treatment. Postwar Polish histories of Ukraine used Soviet categories, but they implied that at least until the twentieth century Ukraine's past was distinct from that of its two most important neighbors.

In the Soviet Union in the 1920s, the party tolerated diverse historical schools, which emphasized the uniqueness of each non-Russian republic. But after 1934 party officials (not scholars) promoted a statist, Russocentric orientation in Soviet historiography. The new "history of the USSR" reflected Stalin's desire "to institutionally merge the RSFSR into the USSR." Soviet historians now "treated the non-Russian regions as integral parts of the larger Russian

whole and minimized, if not ignored, diversity, plurality, and Russian–non-Russian conflicts in the past" (p. 208).

This monograph is invaluable for all who probe the interrelationship between history and national identity change, especially in Eastern Europe, where state-sponsored identities often come into conflict with national and regional identities. By reviewing over eighty Polish and Soviet surveys and over one hundred different monographs, Velychenko has done yeoman work on Polish and Soviet interpretations of Ukrainian history. Although this monograph is encyclopedic in nature, it is well written, objective, and nuanced.

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DENNIS KUX. *India and the United States: Estranged Democracies 1941–1991*. Foreword by DANIEL P. MOYNIHAN. Washington, D.C.: National Defense University Press. 1993. Pp. xxiv, 514.

Dennis Kux's diplomatic narrative about the ups and downs of U.S.–India relations in the last fifty years posits two familiar questions: why are the largest democracies estranged in spite of so many commonalities in their approaches and outlooks, and why do both nations still need each other, particularly in the post–Cold War period?

In the final years of World War II, India, while still part of the British empire, actively supported the Allies. Yet the diplomatic game between London, Washington, and New Delhi heated up. On the one hand, President Franklin D. Roosevelt gave promises to Mahatma Gandhi, Jawaharlal Nehru, and other Indian nationalist leaders of America's determination to end the last vestiges of colonialism in Asia, but because of the "special" and "strategic" nature of the Anglo-American relationship, FDR had to accede to the wishes of British Prime Minister Winston Churchill. Besides the domestic considerations in which the postwar Atlantic alliance took shape, the United States was not amused by India's stand on nonalignment, which it considered pro-Soviet. With the Cold War rhetoric in full swing during the late 1940s and early 1950s, President Harry Truman's desire to open a dialogue and seek the friendship of Nehru's India had few supporters in Washington.

Indian leaders wanted to maintain cordial relationships with both Washington and Moscow, thus trying to attain maximum leverage in their efforts to industrialize their newly independent nation. India did receive massive aid from both the United States and the Soviet Union while maintaining a neutral stance in various international arenas. In the Korean War, for example, an Indian Army contingent was an important contributing element for the U.S.-led United Nations forces. But in the case of Hungary in 1956 and Czechoslovakia in 1967, India, much to the

chagrin of Washington, sided with the Soviet Union, refusing to condemn Moscow's military actions. The visit of Dwight Eisenhower to India in 1959, in which he was warmly welcomed, helped to develop a chemistry with Nehru. The personal diplomacy of U.S. and Indian leaders also continued in the 1960s, when President John F. Kennedy, a friend of India since his Senate days, welcomed Nehru to Washington during the latter's state visit in 1961. The Kennedy period is still heralded as an important marker in the evolution of U.S.-Indian relations, as Kennedy actively helped India in its war against China in 1962 and continued to provide massive economic aid. Although India's military action in the tiny state of Goa created some unease and tension in Washington, the visit of Jacqueline Kennedy later in 1962 improved relations.

Pakistan, India's arch rival to its west and east (until 1971), has also constituted an important irritant in the U.S.-India relationship. Although Pakistan was willing to be a military and strategic partner in the CENTO and SEATO pacts, India preferred not to enter into any formal military alliances with the United States. Indian leaders also expressed dismay and anguish over American diplomatic support to Pakistan over the Kashmir Valley in the wars of 1965 and 1971, the results of which nevertheless ultimately favored India.

The nuclear issue regarding U.S. support of the Tarapur uranium enrichment plant took a dramatic turn in 1974, when India exploded a nuclear device, which it termed as "peaceful." To U.S. policy makers, this signaled India's determination to be a regional superpower in South Asia, thus endangering the rough strategic parity vis-à-vis Pakistan. For India, however, it was a question of national self-interest and of choosing its options carefully.

With the end of the Cold War and the dismemberment of the former Soviet Union, an element of realpolitik has dawned on Indian policy makers. From the 1980s onward, India has progressively liberalized its tightly controlled domestic market to American multinational corporations, and this new phase of economic partnership may very well be the beginning of a fruitful and productive relationship.

Kux's book is a significant contribution to the intricate and often complex world of diplomatic maneuvers and counter maneuvers as both New Delhi and Washington grapple with the changing realities of the international scene and show a willingness to live in a spirit of harmony and accommodation.

MOHAMMED B. ALAM
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ANCIENT

A. D. LEE. *Information and Frontiers: Roman Foreign Relations in Late Antiquity*. New York: Cambridge University Press. 1993. Pp. xxii, 213. \$54.95.

Two inquiries converge in A. D. Lee's innovative assessment of international relations. The first centers on the movement of information across the imperial frontiers. What did the Romans know about Sasanian Persia or the Germans during late antiquity (third to seventh centuries)? How did they acquire their information? Lee argues that the flow of information through both formal (spies, ambassadors) and informal (pilgrims, students, merchants) contacts was sufficient to provide "background knowledge" of Sasanian Persia, whose "imageability" (p. 89) was enhanced by the infrastructure of towns and roads, centralization of political power, and the capital at Ctesiphon. Because the Germans lacked this institutional profile, and both formal and informal movement of information across the western frontier zone was less regular, the "background knowledge" that the Romans possessed was incomplete and uncertain. To determine whether these differences account for Roman behavior in the two frontier zones, or their variable success in maintaining control, a more detailed examination than Lee has provided is required. But he has effectively dispelled the conventional wisdom that the frontiers were information barriers and has made a strong case that "the late Roman government had more information relevant to foreign relations at its disposal than has usually been recognized" (p. 2).

Unfortunately, his definition of foreign relations, the focus of his second inquiry, is too restrictive. By excluding the other frontier zones (Britain, North Africa) because they "demanded less attention" (p. 3)—that is, did not require a military response—Lee has forfeited any hope of providing a comprehensive assessment of Roman interaction. Moreover, his adoption of the conflict model of Roman frontier studies distorts his interpretation of the evidence. One would never guess that the conflicts in the two frontier zones that he has highlighted interrupted long periods of relatively peaceful coexistence. It may well be true that the late Roman government had a "more systematic attitude toward the conduct of foreign relations" (p. 48), reflected in new assignments for imperial bureaucrats and the employment of interpreters, but it does not follow that "interest in foreign relations increased over the course of late antiquity" (p. 140). Neither is it clear that the "strategic information" (knowledge of enemy handicaps or military preparations) that their minions gathered was decisive in the formulation of foreign policies. Roman relations with Persia, for example, cannot be understood without reference to previous relations with Parthia, which had been the bedrock of Ostpolitik since 53 B.C. The "mutual suspicion and fear of one another's intentions" (p. 25) was historic and did not depend on fresh information for inspiration.

Such reservations do not diminish Lee's achievement. He has produced the first systematic assessment of the movement of information across the frontier zones and provided the foundation for an inquiry that

he and other Romano-Byzantine historians must now develop.

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RALPH WHITNEY MATHISEN. *Roman Aristocrats in Barbarian Gaul: Strategies for Survival in an Age of Transition*. Austin: University of Texas Press. 1993. Pp. xv, 275. \$30.00.

Ralph Whitney Mathisen's book focuses on the ways the ruling elite of Gaul reacted to the arrival of the Germans and the collapse of Roman authority. His strategy is largely to let members of the Romano-Gallic elite speak for themselves. After a brief social-historical introduction, he develops a series of themes that explore the various social, intellectual, and ideological adjustments the elites made in the face of Germanic arrival and Roman departure. He documents Gaul's growing isolation, the search for alternatives in church careers and abstract cultural pursuits, and the gradual acceptance that Rome was gone and *Barbaria* was there to stay. The book is rich in short quotations derived from the letters, sermons, law codes, and other documents of the period.

Roman Gaul in the fifth century A.D. was the focus for many of the historical processes that marked the transition from late antiquity to the early Middle Ages. Between the late fourth and the early sixth centuries the Roman administration disintegrated. Christianity completed its victory over paganism, and Germanic groups gradually consolidated their hold over Gaul. Fortunately for the modern historian these transitions were documented by a rich literary production that gave direct voice to the concerns of virtually every major group of elite players.

This abundant, varied literature and the complex historical problems of the region and the period have attracted increasing scholarly attention. Sophisticated literary and cultural-historical models have been applied to both classical and Christian sources. These have been complemented by research in classical, Christian, and Germanic archaeology. Representative of the range of these approaches and concerns are the essays in *Fifth-Century Gaul: A Crisis of Identity*, edited by John Drinkwater and Hugh Elton (1992).

The organization and approach of Mathisen's book raise serious problems. Discussion of the Germanic takeover is too simplistic. The Roman authors may have treated the "barbarians" as a generic category, but the modern historian needs to acknowledge differences among the groups in their political aims, Romanization, and reaction to the Roman order. For this Walter Goffart's *Barbarians and Romans A.D. 418-584* (1980) remains a better source.

Not enough attention is paid to regional differences in Gaul. During the late empire the historical experience of Roman Gauls living on the Atlantic and on the Mediterranean were very different. Temporal changes are not always handled with the clarity that

the process demands. This was a relatively long period of time that saw much gradual change. The grouping of original sources makes for odd juxtapositions that confuse this evolutionary picture.

Although his handling of the great variety of texts is imaginative, Mathisen tends to group them too simplistically into thematic categories, not paying enough attention to questions of genre and rhetorical agenda. This was a highly polemical age. A Christian bishop used the German presence for different purposes than a pagan aristocrat.

The reader not familiar with the complex events and diverse personalities of the period will find this a difficult book. Little historical background is supplied, and authors and historical personalities receive only minimal identification. Those familiar with the period will appreciate the many insights into late Romano-Gallic society but will feel uncomfortable with some of the simplistic juxtapositions. The volume does have full documentation, a good bibliography, a decent index, and maps.

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ROGER S. BAGNALL. *Egypt in Late Antiquity*. Princeton: Princeton University Press. 1993. Pp. xii, 370. \$29.95.

Roger S. Bagnall's introduction to this book should be required reading for all students of ancient history or, for that matter, any field of history. It is a lucid exposition both of the historian's craft and the peculiarities of the field he or she covers. His study presents the researcher into the ancient world with an unusual problem. There is an abundance of evidence. Much of it is papyrological, and the find sites of the papyri are not evenly distributed over the Nile Valley. Nevertheless, the papyri present a picture of social and economic life that is more complete than anything we find elsewhere in the ancient Mediterranean. Moreover, it cannot be ignored with the comfortable assertion that Egypt is unique. By late antiquity, Egypt had become less and less unique. It was part of the east Roman world.

"Late antiquity" for Bagnall is the fourth and fifth centuries. He casts a few glances at the early Byzantine period of the sixth century, but for the most part leaves it alone. He begins with the physical environment: the Nile Valley, torrid in summer and largely under water from late July to late November. It was, by the standards of ancient agriculture, incredibly fertile. The most important crop was wheat, but the everyday diet of the Egyptian was varied, including both vegetables and some meat. One product seems to have been scarce—olive oil—but only by comparison with the rest of the Mediterranean world. One gets the impression that the cheaper but malodorous radish oil had a large share of the market.

The material culture of Egypt gives Bagnall his starting point, and from there he ranges widely. He

touches on government, the disposition of wealth, the production and distribution of goods and services, Greek education and culture, rural labor and slavery, and Christianity and monasticism. The composite picture that emerges is not entirely one that is familiar from textbooks. First, Bagnall makes the point that Egypt was not overburdened by imperial bureaucrats. His estimate, which he considers generous, is one bureaucrat per 5,000 population (p. 66). By contrast, modern New York has a ratio of one bureaucrat per thirty persons for city employees alone. Second, pagan practices survived into Christian Egypt, but pagan temples seem to have been closing their doors many years before Christianity was much of a threat. Demotic Egyptian had ceased to be written, and the native Egyptian who was not bilingual in Greek had reverted to an oral culture. When Coptic appeared, it was an invention of Greek-speaking Egyptians for ecclesiastical purposes. Coptic was a means whereby Christianity might reach a native audience that was not competent in Greek. The development of Syriac from Aramaic is a rough parallel. In both cases, it was Christianity that gave a voice to submerged strata of society.

On the spread of Christianity, Bagnall holds unrepentantly to the conclusions of his article in the *Bulletin of the American Society of Papyrologists* (1982), in which he demonstrated that Christianity was still negligible in Egypt at the end of the third century, but accepted by roughly half the population a quarter-century later. The conclusion may overstate the reality, but, I think, only by a modest margin.

This is a brilliant book, briefer than Michael Rostovtzeff's *Social and Economic History of the Roman Empire* (1926) but worthy of comparison with it. Taken along with the relevant chapters of Alan Bowman's splendid *Egypt after the Pharaohs* (1986), it provides the general reader with a panoramic life of Egypt in the Christian period as revealed by a wide range of sources, most of them papyrological. For the scholarly reader, this book is simply the *Annales* school at its best.

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GARTH FOWDEN. *Empire to Commonwealth: Consequences of Monotheism in Late Antiquity*. Princeton: Princeton University Press. 1993. Pp. xvii, 205. \$17.95.

Garth Fowden's volume fits into a scholarly zone once explored mostly by art historians (beginning with Josef Strzygowski, followed by E. Baldwin Smith, H. P. L'Orange, and Sabine MacCormack). The zone is the problematic of late antiquity: patterns of cognition and expression show themselves either in perpetual flux or in archaizing and resistant rigidity while religion, politics, and society together somehow go on to form unimaginably novel patterns. Peter Brown is a chief practitioner in the new, more eclectic historical scholarship investigating this area, and Fowden

clearly owes something, if by no means everything, to Brown. In six chapters and an epilogue Fowden follows his own main theme of empire moving toward commonwealth, the players being three empires: Romano-Byzantine, Persian, and Islamic. The first and third of these eventually developed into commonwealths or wide areas of cultural and religious unity, but were marked, in comparison to the true empires, by political fission and fragmentation. Fowden sees the motor driving these formations—both empire and commonwealth—as monotheism, a less obvious and more complicated conclusion than it might at first appear.

Fowden must cast a wide net and deploy a careful scholarly apparatus to pull in, organize, and explain the proliferated, interrelated data of his target centuries, between Constantine's imperial revolution and the decline of the Abbasid Islamic empire in the ninth century A.D., and his scholarly ambition cannot be faulted. He has the singular advantage of taking seriously, and using assiduously, the techniques of historical geography, a much neglected but vital discipline for the analytical problems Fowden faces. He deftly brings in those often ignored, far-off extensions of successful Christian mission in Africa and southern Arabia, Axum-Ethiopia, and Nubia, while his dissection of the forces operative in that central arena, the Fertile Crescent, brings this *zwischenland* back to its primal importance as a generator of culture—and of new religions in the late antique period, however defined. Even Mani and Manichaeism, the migrant Nestorians, and Sassanian Mazdaism all get a brief but fair shake in the narrative.

If the spatial dimension in history is especially well drawn in Fowden's work, however, temporal sequence is handled idiosyncratically: both within and between chapters, he makes leaps, reversals, and sometimes awkward and unconvincingly articulated temporal shifts. Rather more important is the fact that, in notable contrast with a particular genus of inflated scholarly work with which we are all too familiar, Fowden's book easily could be a third longer. His conclusions and *obiter dicta*, intriguing and sometimes brilliant as they are, also often call out for expansion, explanation, and an occasional thorough rethinking. Fowden's description of commonwealth is persuasive, for example; his conception of "empire" is less so; and, connected with this difficulty, the ambivalent relationship of the "two eyes of the world," Persia and East Rome, is not quite as well thought out as it could be. His grasp of theological intricacies and tensions is sometimes firm and sure, sometimes not. Fowden's work is innovative and the thought-provoking, for which we should all be grateful, but the reader may simply be provoked when a brisk and penetrating judgment or an incandescent insight is not bolstered by deeper and sturdier supporting arguments.

Still, Fowden's platform of scholarship is essentially sound and solid, his instincts are good, his style is

lively, and the book he has written is by no means a lightweight effort. This volume deserves the attention it will get from historians still trying to decipher the codes of late antiquity.

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MEDIEVAL

KENNETH PENNINGTON. *The Prince and the Law, 1200–1600: Sovereignty and Rights in the Western Legal Tradition*. (A Centennial Book.) Berkeley and Los Angeles: University of California Press. 1993. Pp. xiii, 335. \$40.00.

Like the glosses and *consilia* of the canonists and civilians with which it seeks to come to grips, this learned, dense, tenaciously researched and intricately argued book is not easy of access. As one reads Kenneth Pennington's study, one is not always sure if the current of argument sweeping one forward belongs to the mainstream flowing directly toward its goal, or if one has drifted unwittingly into a merely tributary stream wending its way through a veritable delta of comparable analyses of subsidiary issues. But it would be churlish to make too much of that fact. The complexity of the book reflects the complexity of the overlapping subjects it addresses: the power of the prince (whether papal, imperial, national, or local) and the rights of his subjects; the counterpoint of the prince's power and the rights of litigants in his court (to be summoned, to testify, to present evidence), crucial aspects of what in the Anglo-American legal tradition has come to be known as "due process of law"; the fusion of Roman, canon, and feudal law in a *jus commune*, a tissue of common law and jurisprudence that by the seventeenth century bound European jurists together. And whatever the convolutions of its course, the book's argument bears along with it a rich spoil of alluvial nutriment, the yield of the author's painstaking canvass of a vast body of juristic commentary, much of it as yet unprinted. Medieval and early modern specialists, then, will have to come to terms with this important work. It makes very clear that in the absence of a decent grasp of medieval juristic thinking we are unlikely to attain an accurate understanding of early modern notions of sovereignty and individual rights.

Among Pennington's findings, two may be singled out for comment. The first represents a deepening of his emphasis in an earlier book on the restricted way in which the canonists so often interpreted papal privilege and on the degree to which they refrained from attributing an autocratic power to the pope. The civilians, it turns out, similarly hemmed in the power of the prince (emperor, pope, king) and vindicated against it the rights of subjects. "Paradoxically," Pennington concludes, "absolute power was never absolute." For the jurists invested it "with juridical norms,

natural law, reason, custom, privilege, obligations, in effect, the 'constitution' of the realm" (p. 76).

The second finding speaks to that point, though not without moments of unclarity. It apparently was the great thirteenth-century canonist Hostiensis who first introduced the term *potestas absoluta* into the vocabulary of princely power, placing it side by side with such older terms as *legibus solutus* and *plenitudo potestatis*. In so doing, and in contrasting the pope's "ordained power"—a power limited to the norms embedded in the (positive) canon law—with an "absolute power" whereby he could transcend those limits, Hostiensis had embarked on an exercise in juristic and political theology by borrowing from the theologians a distinction (*potentia dei absoluta/ordinata*) that they had developed in their own efforts to clarify the meaning of the divine omnipotence. In common with this theological counterpart, the legal distinction was to enjoy a continuous career down into the seventeenth century. Not the least of Pennington's contributions in this book is his emphasis on its importance and the degree to which he has enlarged our knowledge of its complex history and our understanding of its shifting meaning. But he is mistaken in concluding that for the theologian God would never exercise his absolute power so that, in borrowing the term, Hostiensis had to transform "a potential into a real power" (p. 69). The theological usage was in fact no more stable than the juristic. Duns Scotus and many another theologian after him understood the *potentia dei absoluta* as involving a real power of extraordinary action. They understood it, that is, in juristic fashion and not without explicit reference to the juristic analogy.

FRANCIS OAKLEY
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JEANNINE HOROWITZ and SOPHIA MENACHE. *L'Humour en chaire: Le rire dans l'Eglise médiévale*. (Histoire et société, number 28.) Geneva: Labor et Fides. 1994. Pp. 287. 38 fr.

The scholastic thinker Peter the Chanter, writing in the late twelfth century, took up the question whether Christ could laugh and concluded that he had the capacity but chose not to. Peter's opinion is expressed at just the moment Jeannine Horowitz and Sophia Menache see as a basic turning point in the history of laughter. The authors characterize the early Middle Ages as suspicious of humor; in the thirteenth and fourteenth centuries, however, inducing laughter became an instrument of moral suasion and social control equal to that provided by the technique of inducing fear. Horowitz and Menache's book complements other recent histories of the emotional and the performative, such as Jacques Le Goff's and Aaron Gourevitch's essays on laughter, Jean Delumeau's study of fear, and Jean-Claude Schmitt's pioneering work on gesture.

The topic of this book is a thoroughly engaging

one, but the execution is disappointing. The heart of the book is based entirely on *exempla*, the moral and often very funny stories used by medieval preachers to enliven their sermons. Horowitz and Menache spend much time repeating the contents of these *exempla*, attempting (quite correctly) to wring social history from them; but they do not provide a discussion of exactly which *exempla* collections they are using, demonstrate (statistically, as they would have to) that the stories they choose are typical, or utilize the literary and psychological theories they repeatedly cite to build a convincing analysis of preaching techniques and audience response. They seem not to know of recent American scholarship such as that of Lester Little on the mendicants, Natalie Zemon Davis on rituals of reversal, and Howard Bloch on misogyny (to give only a few examples) that would lend nuance to their treatment. Although they devote a chapter to clerical views of women, their knowledge of women's history is superficial and marred by mistakes of fact, such as the identification of the twelfth-century abbess Hildegard of Bingen as a Beguine. Moreover, they see the medieval church as oddly monolithic, and, although they mention Mikhail Bakhtin's ideas of the carnivalesque, they fail to explore thoroughly the potential for critique and inversion offered by humor and the responses it evokes. Convincing in their argument that laughter is often a way of preserving the status quo, the authors nonetheless seem to me to oversimplify when they repeatedly characterize mendicant preaching as conservative; surely the values propagated by urban preachers were complex (if sometimes repressive) responses to a new situation, not nostalgia for a feudal and agrarian past.

Despite its limitations, this gracefully written and colorful study is well worth reading. It asks an original and hitherto neglected question: how, why, when, and at whose expense did medieval people laugh and induce laughter?

CAROLINE WALKER BYNUM
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ARMIN WOLF. *König für einen Tag: Konrad von Teck, Gewählt, ermordet(?) und vergessen.* (Schriftenreihe des Stadtarchivs Kirchheim unter Teck, number 17.) Kirchheim unter Teck: The Society. 1993. Pp. 144. DM 25.

"King for a day": Konrad von Teck is, as Armin Wolf rightly insists, probably the least known of all the many medieval claimants to the title of king of the Romans, so insignificant that he does not even merit a portrait in the nineteenth-century gallery of imperial likenesses that adorns the walls of the grand salon of the Römer in Frankfurt am Main. It might thus be asked whether he is worthy of a book all to himself, especially in view of the neglect of many a medieval kaiser by modern historians.

Wolf has labored to unearth every scrap of evidence about Konrad and his later reputation, and he has

clearly enjoyed what is at times a somewhat antiquarian exercise. More important, arguably, is what the election of this German nobleman reveals about the electoral process in Germany at the end of the thirteenth century. There were the familiar tensions following the death of Rudolf von Habsburg, and Konrad's supporters chose him on April 30, 1292, in the hope of preventing another long interregnum, such as had preceded King Rudolf's election. But Konrad von Teck died a day or two later, probably by foul means, and he never had the chance to prove his claim to those who doubted his suitability. Neither was the election typical; rather than being elected in Frankfurt, he was elected in Weinheim and was the candidate of one party only. Adolf von Nassau emerged a few days later as the candidate of the archbishop of Mainz and his associates, and of course he would eventually face a renewed Habsburg challenge. Konrad V, as we might choose to call him, was never crowned.

Wolf enters a plea that he was not the only uncrowned king of the Romans, and his constantly stated aim is to ensure that this figure is entered in the roll of German rulers. It is this passionate wish to honor a forgotten German king that throws the book off balance. It might have been better conceived, on the model of sundry histories of a similar crisis in early imperial Rome, as a study of the year of the four (potential) emperors that began with the death of Rudolf in July 1291, and continued after a ten-month break with the rivalries of Adolf, Albrecht von Habsburg, and, indeed, Konrad; or it might have served reasonably well as a study of a noteworthy nobleman. As it stands, the book is a vastly extended, although rather agreeable, minor footnote.

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MALCOLM BARBER. *The New Knighthood: A History of the Order of the Temple.* New York: Cambridge University Press. 1994. Pp. xxi, 441. \$69.95.

Malcolm Barber has written what should become the standard history of the Templars from their foundation in 1120 to their suppression as an order in 1312. Because the demise of the Templars has already elicited a number of fine studies (including Barber's own *Trial of the Templars* [1978]), the real achievement of this book is its well-balanced synthesis of the order's evolution, in both the Holy Land and Europe, before the clash with Philip IV of France. Readers more interested in the mythical afterlife of the Templars will not be disappointed, however, for Barber provides an engaging epilogue on that subject ("From Molay's Curse to Foucault's Pendulum").

Barber's account of the origin and first generation of Templars is the best available. He incorporates into this account his own earlier work on that especially obscure period and accepts the recently proposed redating of both the order's beginning (1120 instead

of 1118) and the Council of Troyes (1129, not 1128), at which the Templars were formally recognized as a new religious order. Despite lingering and occasionally intense doubts about the legitimacy of a military monastic order, the Templars experienced rapid growth in numbers and resources in the twelfth century. They could field about 600 knights and 2,000 mounted sergeants in the Holy Land, and their network of houses extended throughout western Europe, with the densest implantation in France. So effective—and feared—were their military units that Saladin had all of the Templar prisoners except their commander beheaded after the Battle of Hattin (1187).

The fall of Jerusalem, followed by the failure of the Third Crusade and the diversion of the Fourth, marked a decisive turning point for the order. Although it was able to recruit new members and field fresh armies, its original *raison d'être*—the protection of pilgrims visiting the holy sites—became subsidiary to maintaining the vast infrastructure of an international religious order. Barber's chapter on "Templar Life" illustrates the Templar Rule in terms of the actual practices of the monk-knights, and the chapter on "The Templar Network" explains their logistical achievements, including the operation of their own fleet and the establishment of international financial services. Throughout Barber displays a sensitivity to the lives of individual Templars, from the founder Hugh of Payns and successor grand masters to ordinary knights and such support personnel as carpenters and masons. It is that attention to individual lives in the context of the order that makes this such an accessible and appealing study.

This book rests on a thorough knowledge of the disparate primary sources as well as recent scholarship on the crusades. It is written in a lively style and supported by well-chosen maps and photographs. A paperback edition will certainly find a wide readership.

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JAMES C. RUSSELL. *The Germanization of Early Medieval Christianity: A Sociohistorical Approach to Religious Transformation*. New York: Oxford University Press. 1994. Pp. ix, 258. \$35.00.

To Sherlock Holmes the first principle of good detection was to begin with the obvious. In this book James C. Russell grips the obvious with a grim tenacity and pronounces it a discovery of vast and original proportions. Russell argues that Christianization efforts among the Germanic peoples resulted in a "substantial Germanization" (p. 3) of Christianity. I do not know what I reprehend more in this study: its execrable prose, its shoddy scholarship, or its capacity to misunderstand the very issues it seeks to elucidate. Russell uses the passive voice more times than a lifetime vouchsafes to any author, generally eschews

primary sources except when citing them from anthologies or from other people's notes, scissors and pastes 106 long, indented quotations into 209 pages of text, and omits no opportunity to put turgid jargon where sturdy English would do nicely.

The "world view" of the Indo-European peoples was folk-centered and world-accepting, whereas Eastern mystery religions were soteriological, eschatological, and world-rejecting. The Germanic peoples, at the time of their encounter with Christianity, exhibited a high degree of social solidarity, whereas the urban environment of the Roman world displayed alienation and normlessness, or anomie. All of this being asserted as true, it follows that "The Greco-Roman Christianization scenario in which a religious community fulfills the socioreligious aspirations of a highly anomic society would be dysfunctional if applied to a predominantly rural, warrior, pastoral-agricultural society with a high level of group solidarity. For Christianity to be accepted by the Germanic peoples, it was necessary that it be perceived as responsive to the heroic, religiopolitical, and magico-religious orientation of the Germanic world-view" (p. 4). Russell says that a missionary policy was formulated—by whom I could not quite tell—that had "accommodation" and "gradualism" as its central tenets. The unintended result of this "policy" was the Germanization of early medieval Christianity, although that Germanization was also the consequence of a "deliberate inculturation of Germanic religiocultural attitudes within Christianity" (p. 39). I would have been more careful about labeling as unintended the results of deliberate actions.

Russell, whose training is in historical theology, simply knows too little late-antique, early medieval, and ecclesiastical history to write this book. At one point he says that because sources are notoriously lacking it is all right to compensate for an absence of evidence by building up analogies based on "fundamental similarities" that are "posited" in the work of George Dumézil. Elsewhere he uses the writings of Snorri Sturluson (d. 1241) to illustrate early medieval conditions because Snorri was non-polemical, unlike patristic writers. To discuss Clovis's notoriously controversial conversion Russell adverts, at one point, to Samuel Dill's discussion from 1926 of Hincmar's ninth-century *vita* of the sixth-century Remigius of Reims. He draws some essential facts from a five-times-superseded edition of a college textbook, and he has "qualified" (p. 146) evidence and arguments to the effect that the Franks may have been Romanized because it contradicts the imprescriptible truth that the Franks were busy helping to Germanize Christianity.

There simply was no missionary "policy" in the early Middle Ages. No structures existed to articulate or promote such a policy. Russell cites the famous letter in which Gregory I urged the leaders of the English mission to purify and re-use pagan sanctuaries. This is not accommodation in any meaningful

sense. It refers to a strategy for assembling people but says nothing about what they were to be taught. On the contrary, Gregory was urgently insistent on preserving the purity of the faith. As for sanctuaries, anyone familiar with early medieval sources, beginning, for example, with the *Life of Martin*, can cite literally dozens of pagan shrines that were destroyed.

Likewise, the Germanic peoples were not stable and homogeneous. From the third century to the ninth century the construction yards of state building worked overtime as confederations of people formed, unformed, and reformed. One critical buzzword has eluded Russell: ethnogenesis. And to reify "Germanic" is to fall into a kind of racialism typical of the nineteenth century but distressing at the end of the twentieth.

Among examples of the asserted validity of his thesis, Russell cites four cases that I wish briefly to dismiss. He says—citing J. M. Wallace-Hadrill, not sources—that Germanic ideas of war had to be accommodated. Would it not have been fruitful to ask if early medieval writers were not, rather, commenting on the pervasive militarism of the Old Testament or on the seminal insights of Augustine on the just war? Russell asserts that a devotion to objects such as the cross, relics, the Blessed Virgin, or the real presence of Christ in the Eucharist is emblematic of Germanization. But were not late antique (and are not Orthodox) Christians, who are not noticeably Germanic, devoted to these and similar objects? Russell says the cult of saints "rose to prominence" (p. 43) because of Christianity's encounter with the Germans. But was not that cult "prominent" before Germans were Christians and was/is it not prominent in non-Germanic settings? Finally, Russell says that Roman liturgies were mixed with "indigenous" ones and, predictably, Germanized. Unfortunately, Russell seems to know little of the history of the liturgy in Western Europe. He misunderstands, or perhaps misrepresents, the work of Cyrille Vogel or else he would know that the Romano-Germanic Pontifical is about as "Germanic" as the Book of Common Prayer.

That Christianity changed in interesting and important ways as it spread through and beyond the Roman world is obvious. Christianity in Germanic lands was one flower in a bouquet whose others were Coptic, Syriac, Jacobite, Orthodox, and more besides. Christianity changed people and they changed it in turn. The mutuality and reciprocity of change, ignored or denied by Russell, really cannot be exaggerated. The processes by which those changes were effected are among the most complex, but also important and interesting, to which historians can turn their attention. This poor effort contributes little to an old and worthy scholarly effort but it does confirm the validity of a verse of Alexander Pope's that has become a cliché: "A little learning is a dangerous thing."

THOMAS F. X. NOBLE
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ROBERT BRENTANO. *A New World in a Small Place: Church and Religion in the Diocese of Rieti, 1188–1378*. (A Centennial Book.) Berkeley and Los Angeles: University of California Press. 1994. Pp. xxiii, 452. \$40.00.

This book by Robert Brentano on religious history in the diocese of Rieti in central Italy confirms him as the leading American historian of medieval Italy and encourages one to compare his book with R. W. Southern's classic *The Making of the Middle Ages* (1953). Brentano's ability to use specific documents to convey an idea of change and his respect for individuals in their particulars vivify his rendering of the historical process and in this he rivals Southern. His guarded generalizing and capturing of nuance and substance through apt examples persuade the reader that the author's judgments are valid without conventional appeals to quantification or attempts to verify by finding analogous changes in the research of others on contemporary cities and dioceses. In a manner similar to Southern, Brentano demonstrates that the fixing of the earlier indeterminate diocesan territorial boundaries after the 1250s is related to a revolution in the minds and emotions of medieval individuals.

The author has documented for Rieti and its diocese several changes that will not surprise most medievalists. This area east of Rome in the period from 1188 to 1378 was witness to the appearance of a nascent bureaucracy in the bishop's curia, apparent from the reliance on notaries and written record that replaced the use of oral memory, the overwhelming importance of the clergy in religion, the central role of priestly and canon status within the clergy, the Christocentric nature of piety by the fourteenth century, the intrusive power of the papacy primarily through the pope's power to appoint to clerical offices, and, despite this, a resilient localism even when in the process of transformation.

More importantly, Brentano shows how the influence of St. Francis and his successors, including the other mendicant orders, provided a new model of Christ that transformed the piety of clerics and the laity alike. In his final chapter on testaments Brentano demonstrates that specific individuals acted as Francis's Christ in their bequests to the poor, family, and clergy. An individual's apparent meaningless list of bequests, in Brentano's hands, becomes a vision of charity and a rational expression of his or her historical experience and social place.

The value of this book goes far beyond the importance of its interpretations. Brentano's historical methodology, although he would not use such language and certain aspects of the approach may be specific only to him, should be a model, and not just for younger medievalists. Instructive to all who do medieval history is Brentano's gentle warning that individuals cannot be abstracted to equal value because of the difficulty in knowing all the values and experiences of medieval men and women, although

an institution or groups of humans over time may illustrate a certain definable transformation. And this the author demonstrates in the case of the canons and bishops of Rieti. Brentano has an elegiac reverence for every individual in his history that leads him to see each historical person in his or her specific set of circumstances. Elegantly written and with great respect for and demands on his reader, this book will become a classic of medieval history.

JAMES R. BANKER

North Carolina State University

J. K. HYDE. *Literacy and Its Uses: Studies on Late Medieval Italy*. Edited by DANIEL WALEY. New York: Manchester University Press; distributed by St. Martin's. 1993. Pp. xi, 268. \$69.95.

This book is a hybrid. When he died suddenly in 1986, J. K. Hyde, well known for his work on medieval Italy, had a book in progress on literacy in the later Middle Ages and Renaissance. Daniel Waley, the editor of this volume, found that some of the material Hyde had assembled was in publishable condition and presents it with updated notes in two of the book's eight chapters. One chapter, edited by David Morgan, treats ethnographical literature; the other studies diplomatic reports as a means of news gathering. The remaining six chapters reprint previously published papers by Hyde on related matters: "Medieval Descriptions of Cities," "Italian Social Chronicles in the Middle Ages," and "Italian Pilgrim Literature in the Middle Ages," from the *Bulletin of the John Rylands Library*, 48 (1966), 49 (1966), and 72 (1990); "Some Uses of Literacy in Venice and Florence in the Thirteenth and Fourteenth Centuries," from *Transactions of the Royal Historical Society*, 5th ser., 29 (1979); "Contemporary Views on Factions and Civil Strife in Thirteenth and Fourteenth-Century Italy," from *Violence and Civil Disorder in Italian Cities, 1200-1500*, ed. Lauro Martines (1972); and "Navigation of the Eastern Mediterranean in the Fourteenth and Fifteenth Centuries according to Pilgrims' Books," from *Italian Archeology*, 1, suppl. ser. 41 (1978).

Two themes tie these selections together: the growth of literacy in the High Middle Ages and the shift from clerical Latin authors to lay vernacular ones with an attendant shift in the content and emphasis in each literary genre studied; and a seamless transition from the High Middle Ages to the Renaissance, even in areas where breakthroughs into modernity have been seen by Renaissance historians. Thus, Hyde shows how Leonardo Bruni's *Praise of the City of Florence* can be contextualized amid the other, earlier city panegyrics written as propaganda; and, although it is not mentioned, Niccolò Machiavelli's *Florentine History* would find itself comfortably situated amid the social chronicles and analyses of faction and political instability that occupy two of the chapters. The essays on pilgrim literature accent the nuts-and-bolts information on navigation, maritime hazards, technical

advances in shipbuilding, and shipboard life noticed increasingly by pilgrims, who were men of affairs interested in the political, military, legal, and social institutions of the Near East. The same is true of the ethnographers. In discussing this genre of travel literature, Hyde focuses on accounts of the Mongols and emphasizes the seminal contributions of John of Plano Carpino and Marco Polo and his collaborator, Rusticello of Pisa. He treats these authors as having created a new genre, despite ancient precedents, and as having provided the conceptual models used for the description of non-European peoples in the age of discovery. Other historians seeking to contextualize the latter group of writers have generally pointed to classical *topoi* or to Christian millenarian beliefs; but Hyde makes a good case for his own argument.

Finally, his essay on diplomatic rapportage can be read as a revisionistic critique of Garrett Mattingly's *Renaissance Diplomacy* (1955). Hyde shows that the basic institutions were in place, in Aragon as well as Italy, by 1400, and that the initiatives of patriotic civil servants played as much of a role as those of governments in establishing diplomatic networks. As with his essay on the uses of literacy in Venice and Florence, which also traverses a known landscape, that of diaries, *ricordi*, and personal chronicles, Hyde's concern here is not merely the factual material that these sources provide. Rather, his goal is to highlight what amateur authors wrote, for practical purposes as well as pleasure, to document the expansion of literacy in a register of writing close to the spoken language. He succeeds in doing just that. He also succeeds in challenging some of the *idées fixes* concerning the Renaissance. His editors are to be thanked for bringing together the studies that make up this book.

MARCIA COLISH
Oberlin College

STEFANO PIASENTINI. *Alla luce della luna: I furti a Venezia (1270-1403)*. (Ricerche.) Venice: Cardo. 1992. Pp. 270. L. 52,000.

Stefano Piasentini has assembled and analyzed all the extant cases of theft prosecuted by the *signori di notte*, one of Venice's many police patrols, for the period 1270-1403 and for the years 1486-90 and 1516-23. He has supplemented that material with other cases prosecuted by the *avogadori di comun*, the state attorneys, before the Council of Forty, and from other government bodies. Meticulously researched, the study provides a wealth of data about the prosecution and punishment, and the perpetrators and victims of theft in Trecento Venice. It says less about the significance of theft and the construction of criminality in an emerging capitalist economy.

In the first two chapters Piasentini details the procedures and competencies of the magistracies involved in the prosecution of theft and explores the various criminal acts that were construed as theft, including robbery, piracy, fraud, and even sacrilege in

the case of theft of religious objects. The system was clearly stacked against the defendant, with only two of more than twelve hundred cases examined ending in acquittal. Between 20 and 30 percent of prosecutions resulted in the death penalty, and the severity of punishments increased over the course of the Trecento.

In the third chapter the author provides a social analysis of theft. Thieves were overwhelmingly male; between 1270 and 1346 only 20 percent were women; between 1348 and 1403 that number declined to 12 percent. Often women, many of whom were slaves, servants, or prostitutes, were accomplices in theft and were lured to crime by sentiments. Males, most of whom were *salarati* or workers from the lower ranks of society, were driven to steal by drinking, gambling, or desperation. Only one nobleman was condemned for theft, although several noble bastards were prosecuted. Overwhelmingly, thieves were foreigners; 70 percent of those prosecuted were non-native Venetians. Piasentini uses to good effect the many nuggets of information about social life that the trial records reveal.

Additionally, Piasentini considers the rhythms of crime and searches for a correspondence between the incidence of theft and the economic condition of the city. He finds that there was a relationship between the availability and cost of food and the rate of theft. But here and elsewhere the quantitative approach limits the questions that the author seems willing to pursue. For example, although he states that Venetians collectively suffered from an obsessive fear of theft (p. 115) that was easily awakened, he never explores how various police forces and magistracies used the judicial process to construct a vision of theft and of its perpetrators or what significance thieves (and victims) attached to the objects they stole. The court records are largely accepted as a transparent record of wrongdoing.

This book is a useful contribution to the growing literature on Venetian criminality and justice. A helpful appendix with a summary of cases is included.

DENNIS ROMANO
Syracuse University

DENISE ANGERS. *Le terrier de la famille d'Orbec à Cideville (Haute-Normandie) XIV^e–XVI^e siècles*. Foreword by HENRI DUBOIS. (Société de l'Histoire de Normandie, number 77.) Montreal: Presses de l'Université de Montréal. 1993. Pp. 299.

Denise Angers provides here an edition with analytic introduction of a fifteenth-century *terrier* from the area of upper Normandy. This *terrier* is of interest because it is a survey of the landholdings of a private person who is also only a petty seigneur. Rarely have such documents survived. The original document was prepared in 1429–30 but was annotated from time to time into the sixteenth century. The time of the *terrier*'s composition corresponds with the turning

point of the Hundred Years' War. Normandy had been hard hit by the English invasion and Georges d'Orbec (who had the *terrier* prepared) had found it necessary to swear loyalty to the king of England in 1420 only to return to allegiance to Charles VII in 1450. Perhaps the *terrier* was prepared to justify d'Orbec's property claims in such changing times.

The seignury described in this *terrier* was principally created between 1380 and 1413, first by means of an advantageous marriage, then further by taking advantage of other men's difficulties resulting from the unsettled times. The seignury thus created was rather small but nonetheless very complex. Some of the property was held directly from the king; some of it was held of other seigneurs. Likewise, the properties were not all contiguous. The heart of the seignury was centered on the village of Cideville, but there were four other scattered parcels containing, within their boundaries, holdings claimed by other seigneurs.

The *terrier* (written in fifteenth-century French) is important for the insight it gives into relations between the seigneur and his tenants and among the tenants themselves. Georges d'Orbec is revealed as a wise and active manager of his tenements. If conditions were too bad, he reduced rents. If the tenement had been abandoned, he either absorbed it into his domain or tried to reassign it in a way that contributed to creating blocks of land for other tenants.

The *terrier* is not very helpful for establishing the demography of the region since the tenancies were composed of scattered holdings and the same individual might be a direct tenant for one holding, a subtenant on another, and have subtenants of his own on still others. It is also clear that some tenants held properties from other seigneurs. Furthermore, the size of the holdings varied greatly. At the time the *terrier* was first composed there were relatively few tenants whose holdings were significantly large, whereas most of the holdings were relatively modest. By the sixteenth century, when this *terrier* ceased to be used, there had been considerable change. Some of the tenancies had been divided a number of times among heirs with the plots becoming smaller each time. Others had increased in size through advantageous marriage, by purchase, or by exchange, in an effort to increase the holding and to bring closer together the widely scattered plots. The overall movement was to increase the number of rather large tenancies but also to increase the number of very small holdings.

This *terrier* indicates that this part of Normandy was still suffering from the effects of the Hundred Years' War and from the continued parceling of holdings. Only the largest holdings were able to remain intact and take advantage of the weakening position of the smallholders.

Angers's edition and commentary make a valuable contribution to the increasing amount of material

available for the study of the peasantry in late-medieval France.

K. F. DREW
Rice University

MODERN EUROPE

BARBARA B. DIEFENDORF and CARLA HESSE, editors. *Culture and Identity in Early Modern Europe (1500–1800): Essays in Honor of Natalie Zemon Davis*. Ann Arbor: University of Michigan Press. 1994. Pp. x, 280. \$49.95.

This book originated in a symposium at Boston University in 1990 in honor of Natalie Zemon Davis. The twelve essays by her former students exhibit the fruitful intellectual influence of a remarkable historian and teacher. The organizing principle of the book is the formation of identities—spiritual, social, and cultural—a subject on which Davis herself continues to write with originality, learning, and verve. The contributors find their material in France, Flanders, Spain, Switzerland, and Italy, in forms as diverse as religious expression, oral and written speech, urban neighborhoods, printed manuals on women's health care, clerical education, peasant protest, clandestine love letters, and dramatic and historical literature.

Perhaps the finest tribute to Davis is offered by those who do not merely follow her lead in choosing their subject matter and methods but also emulate her intellectual independence. One example: in explaining the use of female masks in the "War of the Demoiselles," Peter Sahllins turns on their heads both Max Gluckman's famous thesis that taking female disguise was a way of reaffirming the social order (*Order and Rebellion in Tribal Africa* [1963]) and Davis's opposing and equally authoritative argument that it stood for social inversion ("Women on Top," *Society and Culture in Early Modern France* [1965]). In the Ariège, Sahllins argues, assuming the guise of women was neither stabilizing nor subverting; instead, by identifying with the female nature of the forest, male rebels protested the Forest Code of 1827 and the state's usurpation of the traditional rights of both male and female members of the peasant community.

Two other pieces are worthy of special notice: Andrew Barnes's study of the social and religious effects of changes in the education of the French clergy, the most historically significant contribution; and Elizabeth S. Cohen's microhistorical analysis of an illustrated love letter in seventeenth-century Rome, the essay that best evokes a particular social and cultural milieu. On reflection, the essays I liked most were those that exemplify still another lesson from the work of their teacher: that sophisticated theory and critical method are tools of good historical writing, but the less fuss about them in the finished product the better.

The editors round out the volume with an intro-

ductory essay on Davis as a historian (where, appropriately, they discuss theory and method) and a concluding, exhaustive bibliography of her writings.

DONALD WEINSTEIN
University of Arizona

PETER BURKE. *The Art of Conversation*. Ithaca: Cornell University Press. 1993. Pp. viii, 178. Cloth \$39.95, paper \$13.95.

In this wide-ranging but misleadingly titled little book, Peter Burke has assembled five essays, most published before. The title refers only to the fourth essay; the others treat particular aspects of "the social history of language," the subject of a volume Burke co-edited with Roy Porter in 1987 (and to which the first paper included here served as introduction); the survival of Latin in the early modern period; "Language and Identity in Early Modern Italy"; and "Notes for a Social History of Silence in Early Modern Europe." The last paper and the title essay are the most original, but all treat topics suggested and enlivened by mixing historical inquiry with anthropology and linguistics. All draw on a wide range of scholarship, contain colorful material to illustrate their subjects, and are organized in a clear and commonsensical way that makes them easy to approach. Given the interest of the topics and the care with which the material has been assembled, it is disappointing to report that the essays turn out to be rather flat and inconsequential.

Part of the problem with this book has to do with the subject. Language belongs to every human activity, and there are times when the social history of language threatens to become the social history of everything. The interpretive questions and quarrels native to the topic are big and broad, and a fair-minded historian who tries to give all schools their due has to work hard not to fall into a bland and murky eclecticism. Burke does not always work hard enough, being content to turn his pieces into long strings of examples (many interesting enough in themselves) offered on behalf of different and sometimes contradictory—but hardly surprising—theses about language and society. For instance, in chapter 1 he notes that different social groups use language differently, but that the individuals who belong to these groups can alter their ways of speaking to fit different situations; language therefore both "reflects" and "shapes" society and culture. As he moves from one of these "points" (as he calls them) to another, he provides such an unwieldy variety of established or asserted relationships between language use and social forms or institutions that the social history of language comes to look more like a collector's box or a catalog than a narrative. This may not be an unfortunate outcome in itself, but it is not wholly clear that Burke takes any notice of the difference.

There are a few places in these essays where linguis-

tic practices possess some clear and definite—but hardly unpredictable—relationship to circumstances of time or place. Thus, early modern British conversational forms appear to have been more direct and less ceremonious than Italian or French ones, and the eighteenth century saw a shift away from ways of conversing that implied hierarchy and toward ones that sought to be or appear more egalitarian. But just as often the social implications of linguistic choice turn out to be contradictory and unstable, as exemplified by the opposing meanings of dialect speech in Italy: “national” because “popular,” but divisive because regional.

The same complexity of meaning characterizes the striking topic of silence, which can mean respect or resentment, submission or stubborn resistance. Burke may be right to speculate that the early modern period saw a “rise of silence,” possibly associated with the “saving” that fed early modern capitalism, and that the forms and meanings of silence began to diverge in northern and southern Europe during the same era. But I suspect that the patterns of change are more complicated and intricate than this, and that even if satisfying patterns could be discerned at a high level, we would probably learn as much about the social meanings of language from the exceptions to them as from examples that follow the rules. Instead of collecting large numbers of instances in the hope that they will fall into some kind of order, the topic of silence might be much more interestingly explored by developing a sustained and “thickly described” contrast between, say, Quaker silence and that of Sicilian villagers (both of which Burke mentions in passing). In this way we could begin to map the ways particular groups and individuals find to realize the varied potentials that silence, like specific forms of speech, contain for dramatizing or intervening in social relationships. It is on this more local level, and with a more focused and probing method, that the social history of language could begin to come to life.

JERROLD SEIGEL
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MARTIN WARNKE. *The Court Artist: On the Ancestry of the Modern Artist*. Translated by DAVID McLINTOCK. (Ideas in Context.) New York: Cambridge University Press. 1993. Pp. xx, 299. \$54.95.

At least from the time of Zoser and Imhotep, people we would now call “artists” have been indispensable for the realization of the forms of state rule, inventing and constructing the spaces, images, rituals, and spectacles of power. The Enlightenment definition of art as “aesthetic” and “purposeless” yielded a correlative internal art history to which the deep affiliations of art and power were merely incidental. In this excellent book, Martin Warnke traces modern notions of art and artist precisely from their social and political origins in the late Middle Ages. The changes he describes are pan-European, and it is in the

aristocratic courts, not the cities, that the modern “emancipation” of the artist began. We have failed to see this, Warnke argues, because bourgeois history has claimed the “rise of the artist” as a symbol; in reality, however, the artist was not so much an autonomous individual inspired by the spirits of nations and peoples as a functionary conspicuously favored by princely largess.

The deep patterns for the developments Warnke describes were clear by the mid-thirteenth century, when courts had become fixed administrative centers and rulers began to include artists in their immediate court circles. Artists performed a variety of services, some trivial, others important. Some artists—many over the centuries—were granted titles. The courts provided the framework within which the Renaissance style spread. The cities, where the power and values of the guilds prevailed, were generally conservative in artistic matters, and artists were licensed as artists above all by their accomplishments, and by praise of their accomplishments, at court. By the time of Leonardo da Vinci, Michelangelo, and Titian, agents of the mightiest monarchs angled for their services. The *fama* of artist and patron had become closely intertwined, and the *liberalitas* and *magnanimitas* of princes and kings was identified with the patronage of great artists.

Much of the latter part of Warnke’s book is devoted to the explanation of artistic innovations that would prove to be representatively modern in terms of the overarching thesis. The work “beyond price” was a courtly ideal, again distinct from the contracts of the guilds, and lineal ancestor to the “pure” work of “absolute” value. Portraits—the first artistic specialization—began as part of the traffic in dynastic images among the courts, and the new support of canvas—still synonymous with “painting”—made these images more easily portable. Speed of execution (in which personal style, *maniera*, is more visible) was encouraged by the demands of court life and by deadlines much shorter than the long contracted obligations of the guilds. There were also what might be called compensatory innovations traceable to the same conditions. Engravings solved the problem of the isolation of the court and of the immurement of great works in princely *studioli*. Taste at court favored the personal, exotic, and novel, but artists expressed the desire for popular rather than just elite acclaim. These institutional patterns lasted to the French Revolution and beyond.

DAVID SUMMERS
University of Virginia

JEROME BLUM. *In the Beginning: The Advent of the Modern Age; Europe in the 1840s*. New York: Charles Scribner’s Sons. 1994. Pp. xx, 405. \$30.00.

The scholarly debate about the origins of modernity in Europe has been going on for a long time. At first there was widespread agreement that the modern age

began with the French Revolution, the downfall of the *ancien régime*, and the introduction of representative political institutions. Then came those who maintained that the middle years of the nineteenth century marked the great breakthrough, with the achievement of national unification and the triumph of cautious, middle-of-the-road parliamentarianism on the Continent. And more recently a few writers have argued that the *fin de siècle*, by its rejection of accepted conventions in thought and culture, signaled the opening of a new era. The literature on the subject is both contentious and extensive.

Now Jerome Blum has further complicated this debate by suggesting, in a book published posthumously, that the 1840s were the beginning of the modern age. It was a decade of revolutionary change in industry and communications, when the welfare state first emerged, radical social movements grew and proliferated, nationalism took on the menacing tones of chauvinism, romanticism gave way to realism, natural science began to provide a new understanding of the physical world, and social science initiated the application of the techniques of mathematics and physics to the study of society. Blum's case for the 1840s is by no means unpersuasive.

Blum avoids excessive polemicization, and that is all to the good. He prefers to concentrate on capturing the flavor of the period, its aspirations, hopes, ideals, achievements, weaknesses, and failures. He is at heart more of an artist than an analyst. The first four chapters deal with economic, social, and cultural developments. What Blum has to say may not always be new, but it is said with so much charm and grace, with such panache and vividness, that even the most hard-boiled readers are likely to be captivated in spite of themselves. On almost every page there are illuminating, sparkling vignettes, not only of monarchs and prime ministers, but of industrialists, financiers, bureaucrats, social reformers, scholars, poets, frauds, and windbags. This is—dare I say it?—great fun.

The second part of the book, although still lively and informative, is somewhat less effective. It deals in successive chapters with Great Britain, France, Austria, Germany, and Russia in the 1840s. The radiant style, sharp insight, enlightening detail, and masterly touch are still there. But the narrative concentrates too heavily on the traditional and familiar, on kings and statesmen and generals. The treatment is a little superficial. Only in his description of conditions in Russia does Blum look beneath the surface of monarchical politics and international diplomacy. Even in dealing with Austria, whose history was so familiar to him, he describes, albeit with his usual verve and artistry, events well known to all students of the period. The lack of originality in either research or interpretation is the underlying weakness of this fascinating book.

Still, Blum's work is definitely worth reading, not only by the general public but by professional historians as well. It represents a genre of historiography

that has now almost disappeared from the world of scholarship. It evokes a vanished era when the wall separating the amateur from the professional, the academic from the nonacademic, was not as high as now, before specialization erected an almost insurmountable barrier between institutionalized learning and the educated, intelligent reader eager to know more about the past. The appearance of a book that demonstrates so convincingly that this barrier can occasionally still be overcome is most welcome.

THEODORE S. HAMEROW
University of Wisconsin,
Madison

FRIEDHELM BOLL. *Arbeitskämpfe und Gewerkschaften in Deutschland, England und Frankreich: Ihre Entwicklung vom 19. zum 20. Jahrhundert*. (Veröffentlichungen des Instituts für Sozialgeschichte.) Bonn: J. H. W. Dietz. 1992. Pp. 685.

The strike wave of the 1880s and 1890s marked a turning point in the history of the industrialized West. Europe's twenty-year cycle of revolutions gave way to a new pattern of periodic mass strikes and organized workers' demonstrations. Modern labor unions, industrial relations, interest-group representation, and state social policy all date from these years. In comprehensive detail, Friedhelm Boll addresses the international significance of these changes.

Boll's originality lies in his use of comparative methods and his framing of the problem of unionization within the nexus of wage movements and strikes. He compares Germany, Britain, and France to explain why broad similarities in cross-national strike waves resulted in enduring differences in national labor movements. He first establishes the context of strike waves from 1870 to 1920, overcoming problems of uneven and incomplete comparative data and compensating for limitations in the quantitative analysis with exemplary case studies. He then explores the roles of union, employer, and state institutions, of the business and yearly economic cycles, and of political initiatives, especially the first international May Day in 1890, in the strike wave of 1889–93. He concludes with a study of construction workers in London, Paris, Hamburg, and Berlin, who played a vanguard role in initiating unions and strikes. Boll rejects the artificial distinction between workers' history from "above" and "below" and instead reconstructs how workers' local practices, customs, and associations influenced and were transformed by national/international trends and organizations. Noteworthy is his multilayered analysis of the origins of May Day as a *Fest der Arbeit*. Important variables in explaining national differences are the strike practices of workers and unions, the development from journeymen's organizations to unions, contrasting forms of labor mobility and related living patterns and customs, employers' strategies, and the state's policies toward labor.

Comparative analysis undermines many commonly

held generalizations. French workers participated less in strike waves before 1920, despite syndicalist rhetoric; British workers led in most strike categories, despite moderate unions and collective bargaining; German workers struck massively and organized unions more exclusively around wage movements and strikes, despite Social Democratic misgivings. Moreover, theories about the "modernization" of strikes and the decline of strike waves after unionization receive little support. "Weak" groups of nonunion workers, like textile workers, struck as often with as much organization, rationality, and success as unionized craftsmen, while the traits advanced to explain the "modernization" of strikes pervade the preunion and even preindustrial era. Industrialization entailed no simple transition from "tradition" to "modernity," but rather complex adaptations and redefinitions of informal (and poorly documented) preindustrial labor practices from which better-studied (and documented) unions innovated.

Why did German, British, and French workers develop strikingly different labor organizations and systems of industrial relations despite broad similarities in cross-national strike waves? Boll rejects economic and industrial structures as an explanation; they differed little across the countries at this time, and workers in technologically backward industries often led unionization and strikes. The impact of state institutions and policies offers a more promising explanation; still, workers in Germany and France responded to similar state repression of unions in diametrically opposed ways. Boll emphasizes "the different historical preconditions" of labor movements in "their formative beginning phase" (p. 620), that is, the historical-political conjunctures and pre-existing forms of organization and mobility (especially of the labor market and of journeymen) at the moment of initial unionization. Although similar economic conjunctures provoked simultaneous strike waves across countries, varying political and labor contexts ensured contrasting long-term consequences for unions.

As a first try at systematic comparison, Boll's study raises as many questions as it answers. Oddly, Boll does not treat in depth the subject that inspired his study—the international strike wave of industrial workers around 1890—nor does he discuss the relationship of their "new" unions to established craft organizations with the same acuity he devotes to the influence of journeymen on craft unions a generation earlier. Combined with his omission of a systematic analysis of economic/industrial structures, these gaps serve to emphasize continuities and to ignore ruptures and innovations in workers' history more than is warranted. Nevertheless, Boll has made a major contribution to comparative history. He shows great skill in resolving the problems posed by different national historiographical traditions and levels of documentation, most notably by focusing his extensive original

research to make existing national studies and strike surveys comparable. His approach could productively be extended to other countries and time periods.

LARRY PETERSON

City University of New York

JEREMY BLACK. *Convergence or Divergence? Britain and the Continent*. New York: St. Martin's. 1994. Pp. xi, 316. \$45.00.

Jeremy Black's valuable book discusses Britain's complex relationship with the European continent from Roman times to the present. Black is a specialist in eighteenth-century British politics. The present work, a collection of loosely linked brief abstracts (at times no more than a sentence long) of a great number of books and scholarly articles, is a useful contribution to the growing literature on the subject.

We miss fuller discussions of important questions for which Black's introductory chapter specifically rouses our appetites: the stress on England's exceptionalism, on which the British historical profession has traditionally established itself; the place of Ireland and Scotland in writing "British" history; or the usefulness of considering some regional configuration (such as northwest Europe) as a context for British history, more or less on the model of Fernand Braudel's portrait of the Mediterranean world. But these matters, although sporadically alluded to in the body of the volume, receive no substantial attention either there or in an all-too-short concluding chapter. Perhaps to have gone beyond an outline of previous scholarly discussions of the subject, however, would be more than one should expect from such a work.

Black can hardly touch on every subject, and consequently readers will feel that some favorite topic has been omitted or insufficiently explored. One may regret, for instance, the comparative neglect of such an important issue as the divergence in the nineteenth century between British and Continental views of freedom of trade and of the seas; both free trade and Britain's wartime reliance on naval action against neutral commerce cast it as an overbearing exploiter and maritime despot in Continental eyes. And, on a more obvious and more trivial note, it would have been instructive had Black given some attention to the contrast between Britain's apparently "irrational" insistence on retaining its medieval system of weights and measures (and of coinage) while the Continent proceeded along metric (and decimal) lines.

In a final chapter, Black links his enterprise with Britain's relationship to the European Community. In a tentative summation, and on a note of compromise, he argues the utility of a Britain that is part of Europe but retains "the sense of the living past that is such a vital component of a people's understanding, acceptance and appreciation of their own society and

identity" (p. 270). Black thus concludes by plumping for a moderate exceptionalism.

BERNARD SEMMEL

City University of New York

ILANA KRAUSMAN BEN-AMOS. *Adolescence and Youth in Early Modern England*. New Haven: Yale University Press. 1994. Pp. xi, 335. \$32.50.

The condition of children, even more than the condition of the poor, is the real test of a society's quality. It is a measure of the commitment to all and not just to one group. And if we are neglected when we are only potentially human, that neglect will decisively affect us as adults. So it must show something about our own time that the history of children is so neglected. Ilana Krausman Ben-Amos not only makes a solid contribution to the subject but also illustrates how far we have to go.

The study of "adolescence and youth," as she demonstrates, is still largely anecdotal. It still concentrates on the institutional provision for children because of the availability of records on schools, orphanages, crime, and, in this case, apprenticeship. And it is still somewhat unclear how it should be linked to a history of adulthood.

Ben-Amos has nothing new to say on the cultural history of "childhood," concentrating rather on the social history of children, or of that minority of children who were apprenticed. She gleans statistical observations from a wide range of secondary treatments of the larger economy, and supplements them by work in the Bristol city archives and from about seventy autobiographies (four by women). Bristol offers her a perspective distant from London.

If the study generates few surprises, it does give us more confidence in our previous picture of the lives of apprentices. We learn more about all of her subjects, including childhood tasks, death of parents, long separations and mobility, aid from kinfolk, adjustment to apprenticeship, continued parental backing, and graduation to accepting apprentices of one's own. The instances she describes show the range of "possibilities," although only statistics would show the "likelihoods" or the "expectations" involved.

Using biographies of persons born from 1546 to 1793 should have allowed more sense of development over time than Ben-Amos gives. She does not take much account of regional variation, sectarian differences, or the situation of youths who did not enter apprenticeship. She makes what she can of the apprenticeship of girls. And we are reminded of the considerable differences between the experience and the prospects of, say, soapboilers, watchmakers, and merchant tailors.

As for larger questions, Ben-Amos develops the concept of a gradual and—in Margaret Mead's term—"continuous" process of maturation, as opposed to the old notion of a sudden adulthood. She nicely captures the ambiguity of society's expecta-

tions, which demanded not only deference but also a show of independence. And she shows the reasonableness of parents and sympathy between generations, which some have denied.

Where more needs to be done is in seeing child-rearing and training in the context of a developing society, economy, and culture. How did this particular socialization program relate to the maintenance of a "traditional" society? Were its peculiarities conducive to changing that society in a more entrepreneurial direction? How did apprenticeship affect those children who were left out? And did any of this relate to the sentimentality that began to dominate English culture toward the end of that period?

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FRANCES E. DOLAN. *Dangerous Familiars: Presentations of Domestic Crime in England, 1550–1700*. Ithaca: Cornell University Press. 1994. Pp. xiii, 253. Cloth \$37.95, paper \$15.95.

The popular literature of early modern Europe, such as newspapers, pamphlets, and chapbooks, has increasingly attracted scholarly attention. The questions scholars raise vary, but several have been interested in the gender relations this literature suggests. The latter group, which includes Frances E. Dolan, has concluded that popular literature in early modern England rejects the gender bias of the age. In separate chapters devoted to various sorts of domestic violence, infanticide, and witchcraft, Dolan argues that gender played the most important role in the social perception of domestic crime in early modern England.

Unfortunately, Dolan's use of primary sources raises problems in method. The time period Dolan discusses is enormous, yet she makes little or no differentiation between the literature of early and later periods. Gender relations, like political or economic relations, were not static. Also, all too often Dolan seems unaware of class distinctions that were important to the chapbook's author. Additionally, source selection seems random. Dolan examines some chapbooks and some fine literature, but she presents no systematic explanation for her specific choices nor explains why the language used by Milton, Shakespeare, and street publications will be treated similarly.

Biased readings of some of her primary sources also mar this book. When Dolan discusses the pamphlet *The Bloody Husband* (1653), for instance, she states that Adam Sprackling murdered his wife and then further generalizes that Sprackling "dispassionately" "calculated" using an insanity defense because "legal, literary and moral tests so consistently attributed murderous husbands' actions to madness" (p. 103). Dolan's conclusion that Sprackling "manipulates the convention of the mad husband" would lead the reader to think that insanity pleas were generally

successful. Dolan neglects to mention that in this case Sprackling was executed despite considerable evidence that he was indeed insane. Sprackling was a pariah, a man who regularly drank to excess and was so hated by his neighbors and many creditors that he was unable to leave his estates. When he finally mutilated his wife's murdered body, tortured his servants, cut his beloved dogs into pieces, and smeared the blood all over himself, society had enough of him, insane or not. Dolan places the story in social contexts that have little to do with the written account. The contemporary reporter reflected little, if any, gender bias despite his being solicitous of Mrs. Sprackling's sincerity and innocence and Adam Sprackling's obvious insanity and guilt.

The above notwithstanding, there is still much to praise in this work. Dolan's command of the secondary literature is enviable. The voluminous footnotes (conveniently located at the bottom of the page rather than at the end of the chapter or book) are filled with valuable information. Anyone even remotely interested in the literary expressions of the social issues under discussion should read this book. One may not agree with Dolan's methods or conclusions, but the information she has carefully amassed is thoughtful and valuable.

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H. LARRY INGLE. *First among Friends: George Fox and The Creation of Quakerism*. New York: Oxford University Press, 1994 Pp ix, 407. \$45.00.

In 1972 the Quaker historian Henry J. Cadbury stated, "There is no real biography of George Fox." To be sure, a modern, balanced, and scholarly work aware of all the sources on this pivotal figure of seventeenth-century English sectarianism has long been needed. H. Larry Ingle's biography claims to fill the gap. To his credit, Ingle has cast his net wide in search of early Quaker (and non-Quaker) sources. He asserts that his work avoids the pitfalls of older "filiopietistic" Quaker accounts that are a "virtual paraphrase" of Fox's famous *Journal*, first edited by the Quaker Thomas Ellwood and published by the Society of Friends, or Quakers, in 1694 (pp. vii-viii).

The importance of this biography is its intricate retelling of the story of George Fox's life (1624-91) from his early religious experiences in Drayton-in-the-Clay, Leicestershire, through his eventful adult life. Ingle can be credited with giving a detailed and interesting account of the James Nayler and John Perrot episodes. He portrays Fox's opposition to "steeplehouse" worship, the hiringling priesthood, tithes, oaths, hat honor, and other manifestations of what Quakers saw as social and legal oppression and religious conformity. The book focuses on the emerging architecture of the Quaker sect-church, the development of monthly men's and women's meetings, the activities of the Second Day Morning Meeting, and

the Meeting for Sufferings, which together formed the London-based power structure of Quaker elders (by the 1670s), who on occasion censored Fox himself.

Although Ingle has documented the work with full reference to Quaker published and unpublished sources, unhappily his book breaks no new ground but rather follows where others have gone before. The study lacks balance in two main respects. First, Ingle presents his view of Fox without giving a historiographical survey on the state of Quaker studies as a necessary historical context for the biography. Recent scholarship on Quakerism is considerable and growing, but if it disagrees with Ingle's perspective, it receives a mere endnote or is totally ignored. Second, missing is one of the essential tasks in any scholarly biography of Fox, namely, a carefully delineated overview of the conditions and origins of Fox's *Journal* (1694), and the political editing that occurred in its publication. Ingle neglects to note that Ellwood and the Second Day Morning Meeting revised the *Journal* so that "nothing may be omitted fit to be inserted, nor anything inserted fit to be left out." Furthermore, Ingle, who has used primarily the 1694 edition of Fox's *Journal*, does not acknowledge that the first sixteen pages of the original *Journal* covering Fox's earliest preaching of the "Truth" were lost before its first printing in 1694. He fails to evaluate how Fox's *Journal* came to dominate Quaker historiography while other traditions were seemingly suppressed. The reader will find a few obscure endnotes on the reliability of the 1694 *Journal* (pp. 305 n. 95, 346 n. 64, 348 n. 1, 288 n. 7, 319 n. 88).

Ingle opens with the statement that Fox was "founder" of the Quakers, that he was the "First Friend," and that the movement was "his movement." In Ingle's reductionist overemphasis of the role of Fox in founding and organizing Quakers, he has continued the older "filiopietistic" tradition of Quaker historiography, followed by some but not all historians of Quakerism, which has idealized Fox as the "Moses" of the early movement, who came down from the mountain with his own new vision. In assessing the origins of Quakerism, for example, he stays safely away from evaluating the implications of the pre-existence of Quaker-type groups (Seekers) when Fox began his public ministry; the leadership of Francis Howgill, John Camm, and John Audland, among others; the absence of Fox at the important meeting in Balby, Yorkshire; and the work of William Dewsbury in organizing monthly meetings.

Ingle also gives Fox primacy in writing the Quaker "peace testimony" in January 1661. Margaret Fell, who married Fox in 1669, proposed the peace idea in a letter to Charles II six months earlier, and Fox signed it. Fell then personally delivered it to the new king in June 1660. Ingle dismisses this in an endnote, stating that "Fox probably advised her on its contents, for he signed it" (p. 333 n. 16).

Ingle considerably underestimates the role of Fell.

For example, he omits evaluation of her organization of the Kendal fund to support Quaker missionaries and their families in the 1650s. Although he refers to Fell superficially, he neglects analysis of the implications of why her home, Swarthmoor Hall, was the site of the earliest continuous Quaker women's monthly meeting outside London, and why Swarthmoor Hall was a refuge for buffeted Quaker missionaries, a clearing house for all early Quaker correspondence, and a meeting place for important (male) Quaker leaders. A nuanced understanding of Fell's birth, rank, and social status and how it played a significant role in influencing Fox's thinking and promoting Fox's prominence is not well served here.

This is not to detract from Fox as a central figure in early Quakerism, or from his *Journal* as an invaluable source for our understanding of the early Quakers' survival and development from sect to Society. Fox was a remarkable figure, but he also was one of several pivotal first-generation leaders. Had Ingle taken into account the historical problems of the *Journal* and given an adequate historiographical overview of Fox's role based on all the evidence, and then reaffirmed the traditional Quaker view as his thesis, that would have been acceptable, albeit debatable.

Another major flaw of the work is Ingle's failure to a significant degree to make adequate secondary source acknowledgements that professional norms demand, and on which his own work draws. Although he cites them in certain contexts, Ingle all but ignores Barry Reay (*Quakers and the English Revolution* [1985]) and Christopher Hill (*The Experience of Defeat* [1984]; *The World Turned Upside Down* [1972]) on the question of Quaker origins and suppression of evidence, as well as Winthrop Hudson's article on Fox and Elizabeth Hooton, which was the first to raise these issues ("A Suppressed Chapter in Quaker History," *Journal of Religion* 24 [1944]). Neither does he cite Melvin Endy's work (*William Penn and Early Quakerism* [1973]) when he describes the Quaker problem of gradual subordination of individualism for the sake of group survival.

Ingle has made a serious attempt to give us an up-to-date account of Fox's life. Nevertheless, his work would have benefited substantially from a multiple historiographical perspective showing other ways of regarding his subject. Instead, his close association with the Society of Friends proves him unable to question a Quaker orthodoxy or to give attention to alternative viewpoints that act as a counterpoise to his own. Henry J. Cadbury's remark still holds true. George Fox needs a real (balanced) biography.

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LAWRENCE E. KLEIN. *Shaftesbury and the Culture of Politeness: Moral Discourse and Cultural Politics in Early Eighteenth-Century England*. New York: Cambridge University Press. 1994. Pp. xiii, 217. \$49.95.

Lawrence E. Klein's book is part of a growing body of literature that gives ideas about proper behavior the serious scholarly treatment they deserve. It offers an imaginative analysis of the third earl of Shaftesbury's diverse writings, particularly *Characteristicks* (1711) which Klein argues is key to understanding Shaftesbury's achievement. That achievement was, in Klein's view, to adapt ideas of politeness to a legitimization of the post-1688 Whig regime. Thus, Shaftesbury emerges here primarily as a political writer whose concept of politeness is essential for understanding England's long eighteenth century.

The book is divided into two parts, both evolutionary in format. Part 1 charts Shaftesbury's evolution as a philosopher from the production of *An Inquiry Concerning Virtue* (1699) to that of *Characteristicks*. Klein maintains that Shaftesbury's experiences first of challenging the premises in *An Inquiry* and then of grappling with the question of how to reconcile moral autonomy with public sociability culminated in his mature notion of polite philosophy expressed in both the style and content of *Characteristicks*. In addition to challenging his earlier work, Shaftesbury's notion of politeness constituted, according to Klein, an assault on contemporary philosophy and religion. Shaftesbury's polite philosophy was practical and public, not theoretical and cloistered like the philosophy and religion found in universities, libraries, and the church. It centered around ethics, aesthetics, breeding, self-knowledge, and conversation, a combination of characteristics designed to transform the gentlemanly elite and to provide a new basis for culture.

Part 2 of the book analyzes Shaftesbury's political evolution from an opposition Whig in the 1690s to someone who offered a legitimization of the Whig regime in *Characteristicks*. Here Klein emphasizes the political purpose of Shaftesbury's concept of politeness. That purpose was "to attack the Tory loyalty to Church and Court in the name of a new Whiggish culture" centered around politeness (p. 8). Shaftesbury argued that the new Whig regime provided not just the political but also the discursive liberty necessary for culture and politeness to thrive. In his view, the Whigs rather than the Tories were natural custodians of the nation's culture. Their guardianship would result in a shift in the locus of culture from the church and court to the public world of polite gentlemen, a world characterized by free and friendly exchange as well as unlimited criticism. Klein suggests that Shaftesbury, like all Whigs, was concerned with maintaining English liberty, but that he "was exceptional in his serious attention to the culture of liberty and the politics of national and elite manners" (p. 22).

Klein's study will be illuminating to a broad range of scholars, including philosophers, literary specialists, and historians of political thought, religion, the arts, and manners. It highlights Shaftesbury's political purposes but does not ignore his more frequently studied ideas regarding philosophy and morality. The

book's strength is that it evaluates the bulk of Shaftesbury's writings, showing how complex and multifaceted a thinker he was. But if we are to use Shaftesbury as "an important guide to his era" (p. 2), more is needed. It would be helpful to know how the idiom of politeness, so central to Shaftesbury's work and to his age, differed from that of courtesy, which prevailed before the eighteenth century, and that of conduct and etiquette, which emerged later. Or perhaps these were competing idioms existing simultaneously. Placing politeness in the context of other languages of identity and behavior will bring Shaftesbury's era and achievement more sharply into focus.

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MARJORIE MORGAN. *Manners, Morals and Class in England, 1774-1858*. (Studies in Modern History.) New York: St. Martin's. 1994. Pp. x, 196. \$59.95.

This book poses a problem for the fair-minded reviewer. It is bold, intelligent, well organized, and well written. It engages effectively with the secondary literature, while carving out its own distinctive niche. In short, one has every reason to want to say nice things. Unfortunately, its central argument is seriously problematic or downright incorrect. Marjorie Morgan proposes a new explanation for the stabilization of mid-Victorian society in Britain based on her reading of the prescriptive literature on deportment and behavior that was published in the late eighteenth and early nineteenth centuries. According to Morgan, this literature originally took the form of what she terms "courtesy books," grounded in the rituals of court etiquette and aimed at the fashion-conscious gentry and aristocracy. Around the turn of the century, this genre was replaced by evangelical exhortations that purveyed an ideal of pure character and virtuous conduct to the rising middle class. These books, however, were themselves supplanted around 1830 by a third genre in which the preoccupation with inner morality and conduct was replaced by a renewed emphasis on superficial etiquette, comportment, appearance, advertisement, and display.

In the absence of quantitative data on titles, editions, and press runs, Morgan's central hypothesis and chronology is difficult to evaluate. Grounding her analysis in scattered bits of impressionistic evidence, Morgan leaves herself exposed to doubts about these empirical claims. It certainly does not bode well for her argument that the period she interprets as the age of middle-class etiquette witnessed the triumph of the king of all conduct book writers, Samuel Smiles, whose *Self-Help* sold a quarter-million copies by the time of his death. I suspect that the conduct and etiquette books (whose content she describes so crisply) circulated more

or less simultaneously, respectively appealing to people from different middle-class sectors, different age groups, or even to the same individual in a different frame of mind.

If so, her laborious explanation of the transition from stage two to stage three (which occupies the bulk of her book) is a false solution to a non-existent problem. To be sure, Morgan makes many useful points along the way about the necessity of reestablishing new forms of etiquette appropriate to the anonymous "world of strangers" (p. 46) of Britain's new urban industrial milieu. Still, there is a world of difference between the etiquette of traditional court society and the etiquette of keeping "to the right side of the pavement" (p. 100). Morgan's effort to seek the origins in the latter in the legacy of the former requires logical leaps and categorical elisions so acrobatic as to defy synopsis or examination within the confines of this brief review.

She is correct on the larger point that the anomic new world of competitive, urban, industrial capitalism favored (and perhaps required) the development of new forms of self-presentation based on aggressive attention to advertisement, appearance, puffery, and display. Morgan offers many striking examples of this kind of posturing and her astute analyses of them constitutes the best part of her book. Yet her claim that the model for this competitive showmanship was the elaborately regulated rituals of traditional courtly and aristocratic etiquette is neither warranted by the evidence nor consistent with common sense. At an even deeper level, Morgan's exaggerated emphasis on bourgeois display and self-promotion runs the risk (as she half realizes) of distorting the picture of nineteenth-century society: of accentuating consumption at the expense of production and of overdramatizing the element of public spectacle in the way consumption was enacted at the expense of its more prosaic, private, domestic face.

In her first-rate, synthetic final chapter, Morgan virtually concedes a number of these points. The result, however, is oddly to detach her judicious conclusions from the more relentlessly one-dimensional substance in the body of her book. Granted, her Goffmanesque vision of the world as a stage has its undeniable intellectual value and appeal. Duly tempered and incorporated into a more multistranded analysis, historians can (and do) employ it to good effect. It cannot, however, be magically invoked to rescue an argument whose logic is slippery, whose chronology is uncertain, and whose evidential grounding is ambiguous or weak. Morgan asks the big questions of nineteenth-century British history: why did mid-Victorian society stabilize? How did aristocratic culture and power persist? Did the rising entrepreneurial bourgeoisie triumph or default? Could professionalization provide a viable solution to urban-industrial crisis and anomie? On some level she must

know that it will take much more than a study of behavior books to answer them.

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JAMES VERNON. *Politics and the People: A Study in English Political Culture, c. 1815–1867*. New York: Cambridge University Press. 1993. Pp. xviii, 429.

"I have proposed a new narrative which has told a story about the fall of political man, and the closure, not the emergence, of the constitution's radical libertarian democratic potential" (p. 336). And a singularly odd narrative it is that James Vernon has written. This oddness arises less from the scope of his ambition, which is to enlarge our conception of the politics of the first two-thirds of the nineteenth century through an analysis of the diverse languages of communication—oral, visual, and printed—that imbued both official and popular political activity in the localities, than from the theoretical presuppositions informing the ambition. A student of postmodernist critical theory, Vernon has sought to apply its principles for reading a text to the rich variety of forms and actions characteristic of local political culture during the early and middle decades of the nineteenth century.

His principal argument is that reforms of the electoral system usually taken to signify an expansion of the political nation were in fact part of a process effectually devised to restrict access to the public political sphere. Whereas the unreformed "system"—at the parochial, municipal, and parliamentary levels—furnished copious opportunities to most members of the community for direct, ritualistic, and symbolic expression of their political partialities, the reformed structures that took shape in the 1830s and in subsequent decades were much less hospitable to popular participation. Political conflict in the localities, according to Vernon, was over contending definitions of the constitution, and its dynamic increasingly favored an exclusive identification of that constitution with propertied males. Vernon tries to show how both the "privileging" of print over oral and visual forms of political communication and the rise of mass party organization contributed to the "closure" of the public political sphere. Along with the argumentation we are given an abundance of interesting and illuminating descriptive detail, much of it emerging from investigations of the political cultures of Boston, Lewes, Oldham, South Devon, and Tower Hamlets.

One can be grateful for the detail and yet skeptical about the thesis in whose service it is tendered. Although important work by John Phillips and Frank O'Gorman has done something to establish the vitality of the unreformed electoral system, the case pressed by Vernon for that system's expansiveness and inclusiveness goes beyond what the evidence

appears to warrant. Equally problematic is the proposition concerning a contraction of the public sphere in the decades after 1832. It is one thing to say that the proponents of reform within the governing classes held no brief for democracy (it has been a long time since any historian has argued otherwise); it is a different thing to assert that their object was to extinguish the democratic forms and participatory mechanisms of the unreformed order (an assertion that Vernon does little to substantiate). The author himself acknowledges the complexity and ambiguity of many aspects of English political culture in the decades between the First and Second Reform acts, and much that was going on hardly seems consistent with the notion of a shrinking public political sphere. Vernon's judgments and ascriptions of meaning often seem to be dogmatically superimposed on the phenomena he purports to analyze. Statements beginning, "It is possible to suggest" and "It is possible to read" habitually contain deductions that make one think insufficient attention has been given to the distinction between the possible and the prudent.

If the substance is a bit peculiar, the manner of its presentation is strikingly so. Much of the prose is thick and impenetrable. In some paragraphs combinations of words masquerading as sentences consort with sentences whose existence one can only lament. Indeed there are times when the reader is at a loss to know where to put the "[sic]": "Or, one which examines the ways in which politics fails to narrativise the world for us, how politics speaks to our still decentred and fractured selves in such a myriad of contradictory ways that we remained overwhelmed and disabled by our differences" (p. 335). The text is marred by faulty word usage, subject-verb disagreements, misplaced apostrophes, misspelled names, and numerous typographical errors. Cambridge University Press should be less careless of its reputation.

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ROBERT STEVENS. *The Independence of the Judiciary: The View from the Lord Chancellor's Office*. New York: Clarendon Press of Oxford University Press. 1994. Pp. xii, 221. \$37.00.

For the past three decades Robert Stevens has combined successfully the academic obligations of research, teaching, and administration. His book *Lawyers and the Courts* (with Brian Abel-Smith [1967]), has remained a standard history that covered its subject since the middle of the eighteenth century. Another major work, *Law and Politics* (1978), provided a broad survey of judicial politics, administration, and jurisprudence since the beginning of the nineteenth century. Stevens has held appointments at several major law schools and, more recently, served as president of Haverford College and chancellor of the University of California at Santa Cruz. He is now

Master of Pembroke College, Oxford. I provide this brief sketch because Stevens's work, well known to legal historians, has unfortunately gained less notice from historians of modern Britain in other areas of specialization. His lifetime of legal scholarship deserves a better fate.

Stevens's most recent book, with a narrower focus, may not at first sight seem to possess the spacious purview of the books just cited. I wish to emphasize, however, that this work treats topics and provides insights into issues since the 1880s that are well worth pondering by all those interested in the last century of British history. The book's starting point is the establishment of the Lord Chancellor's office in 1884, with a role for a permanent secretary to act as liaison to the legal community and administrator on behalf of the lord chancellor, for centuries the chief legal officer of the crown. How this administrative innovation, undertaken in the wake of the Judicature Acts of 1873 and 1875, affected the frequently cited but rarely analyzed constitutional doctrine of judicial independence makes up the heart of the book.

Originally, in the wake of Stuart history, when judges rejected the role of lions under the throne, judicial independence meant acting as a third branch of government. Eventually parliamentary supremacy eliminated this influence because judges became, as Stevens notes, lions under the mace. The absence of judicial review, in its American form, restricted the authority of judges and relegated the judicial branch to a distant third place behind its executive and legislative counterparts. During the last century, the debate about the place of judges has turned more on the preservation of their social role, a vestige from 1688, rather than permitting the judiciary to become simply another element of the anonymous civil service. As the grant of discretionary power to administrative agencies increased in the twentieth century, the scope of judicial oversight declined. Stevens makes clear that judges of every political persuasion feared the diminution of their status and the loss of the judicial mystique.

Independence has become redefined as distance, a self-imposed withdrawal from the ordinary discourse of public life. Judges have recast themselves in a monastic mold that promotes an air of impartiality. As a result judges have seemed ideal figures to lead royal commissions and politically controversial inquiries. Those associated with Northern Ireland have not raised the estimate of judicial probity. This search for a public position might seem minor, but the role of the judiciary in any scheme of reform, especially the introduction of a written constitution, has major significance.

The mundane issues of judicial pay scales or the tortured paths of selection for the bench reflect broader trends in recent British history well worth contemplation. In summation, therefore, this modest volume deserves an audience beyond specialists in

legal history; it will interest anyone concerned with British history for the past century.

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LYNN ZASTOUPIL. *John Stuart Mill and India*. Stanford: Stanford University Press. 1994. Pp. viii, 280. \$39.50.

This book fills in what was undoubtedly a large lacuna in Mill scholarship: John Stuart Mill spent thirty-five years (or at least large parts of them) at a desk in India House, the London headquarters of the East India Company, yet most scholars have behaved as though he and his thought were unaffected by his professional career. Lynn Zastoupil has seen what now appears obvious, that he was hardly likely to be untouched by the work to which he devoted six hours a day for most of his adult life. Perhaps the gap was left in part because of the prodigious amount of slogging research any scholar attempting to fill it would have to undertake. Zastoupil has devoted himself (with the help of Martin Moir's research and expertise) to identifying Mill's drafts at the India House and relating changing ideas about the governing of India to his intellectual development. The production of official dispatches mandating policies was complicated, "with several people having a voice in their final composition . . . [W]hat survives in Mill's case are mostly hundreds of dispatches in which Mill's views may or may not have been altered by others" (p. 2). But if the amount of research necessary was daunting, Zastoupil's style is untainted by the drudgery. This is an elegantly written work.

The book's six chapters cover different aspects of Indian policy and Mill's thought, especially when he and the century were in their twenties and he was struggling to broaden the ideas instilled by his father. Zastoupil argues that in Mill's dispatches can be traced the changing emphases of Mill's maturing political views. I felt he would have liked to argue that Mill's experience of the success or failure of Indian policies had a greater influence on the changes in his own political outlook than the hard evidence warrants, and he has to content himself with drawing parallels. But I agree with his basic assumption that no man is untouched by his professional experiences and was quite persuaded by the similarities Zastoupil illustrates.

The first chapter sets the scene with a description of James Mill's India House career and the views—in his case unwavering—that he brought to bear on the East India Company's policies. Much needed information about the workings of the office on Leadenhall Street is unobtrusively provided. The discussion moves in the second chapter to James Mill's and John Stuart Mill's views on the education of the latter and of India. Zastoupil next shows J. S. Mill's attraction to the empire-of-opinion school, whose Orientalist sympathies included a common regard "for Indian opinion and maintaining the established order" (p. 57).

This led them to support indirect rule through the reigning princes and to abandon James Mill's preference for direct rule in the interest of establishing efficient administration. Zastoupil draws many parallels between ideas J. S. Mill found congenial in the empire-of-opinion school and those he was known to have imbibed from the "romantic poets and theorists, especially Wordsworth and Coleridge" (p. 76). This theme is convincingly (but perhaps somewhat repetitiously) pursued through the last three chapters: "Princes and Progress," "An Empire of Reform," and "J. S. Mill and the Imperial Experience."

The structure of the book seemed to me determined more by the history of India than that of Mill's thought. Indian events are used to illustrate, through Mill's responses to them, Zastoupil's main theme. In the end I learn less about Mill than about India House and the history of India. But then I had more to learn about them, and I am grateful to Zastoupil for filling, in true utilitarian fashion, two of my lacunae at one reading.

ANN ROBSON
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STEPHEN HOWE. *Anticolonialism in British Politics: The Left and the End of Empire, 1918-1964*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1993. Pp. xvi, 373. \$59.00.

Despite the title and dates, which suggest an examination of the policies of the Labour Party from its refounding at the end of World War I to Harold Wilson's first government, this book is a more narrowly focused study of the far left, root-and-branch critics of any form of imperial control. Basing themselves on the simple and clear principles that empire was evil and bad for Britain and that self-government was an absolute right (p. 309), these small groups opposed trusteeship and preparation for independence as strongly as efforts to maintain full domination. Indeed they were more suspicious, although less openly vocal, of the ambiguities of the Labour government of 1945 than they were of their clearer adversaries, the Conservatives.

Stephen Howe traces various groups—the Independent Labour Party, the Communist Party of Great Britain, the Trotskyites, the League against Imperialism, the Union of Democratic Control (finding a new role in African matters in the 1950s), and many lesser known groups—into the post-World War II period. But the centerpiece of his book is the study of the Movement for Colonial Freedom (MCF), founded in 1954, which Howe sees as the culmination of active opposition to colonialism (p. 231). The MCF was founded by Fenner (later Lord) Brockway, first elected to parliament in 1950, along with other reliable adherents of left-wing causes who thought the time was ripe for a mass movement. The movement had its own journal, *Peace News*, with a circulation of 11,000–12,000, and over three million members (pp.

237–39). Most of the latter, however, were purely nominal and probably even unaware of belonging through their trade unions. By the 1950s, Howe observes, people were generally more interested in consumerism and welfarism than overseas commitments (p. 323). Even in Parliament few members of any party attended colonial debates. About half of the questions to ministers were asked by those belonging to the MCF (p. 251); but the real driving force was Brockway, "a one-man alternative colonial service," often to the uneasiness of his own Labour Party leaders (p. 252).

The Suez crisis, two years after the founding of the MCF, provided ample justification for its activities, but rapid decolonization thereafter did not produce the gratifying results that had been so confidently predicted. Instead of harmony once imperial oppression had been removed, there was all too often factional strife and military coups. Even close to home, in Cyprus, Greeks and Turks could not be reconciled, and members of the MCF were dismayed that the Greeks, with whom they sympathized more than the Muslim Turks, could not be persuaded to adopt Mahatma Gandhi's passive techniques and, even worse, were bitterly hostile to the left (pp. 279–80). But by the mid-1960s anticolonialists were taking comfort in the doctrine of neocolonialism. Independence was regarded as a fraud, and a new villain was found in the United States. The fulfillment of the great aim may have brought disappointment, but Howe insists that the real success of the persistent criticism of the Left for half a century lay in its indirect influence on Parliament, the media, and public opinion and in establishing by proxy the accountability of the colonial rulers to their subjects.

This is a sympathetic but by no means uncritically approving account of the most determined opponents of empire. Although as a revised dissertation it is perhaps inevitably rather densely packed, it contains a prodigious amount of information on the various groups, individuals, and arguments in a relatively small compass. Since it is likely to remain the standard work on the subject for a long time to come, it is a pleasure to commend the publisher for making the notes so easy to consult by printing them at the foot of the page.

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ISAAC KRAMNICK and BARRY SHEERMAN. *Harold Laski: A Life on the Left*. New York: Allen Lane; distributed by Viking. 1993. Pp. xii, 669. \$35.00.

In theory at least, Harold Laski shared the Left's critical aversion to biographical, autobiographical, and memoir literature. Reviewing the *War Memoirs* of David Lloyd George, a political enemy of long standing, Laski decried the reductionism and distortion inherent to historiography as personal narrative: "He still thinks of history as primarily made by men who

can with a given end once, if they have the necessary capacities, secure its triumph . . . The vast, impalpable forces in the grip of which men so largely are do not appear to concern him. Like the supreme actor that he is, the passionate self-absorption in his role enables him to act all such rivals off the stage" (*New Statesman and Nation*, October 3, 1936). This critique did not, however, prevent Laski from reading, collecting, and reviewing political biographies, nor from aspiring fervently and tirelessly to play, in the words of his wife, a "center stage" role in the political dramas of his own era.

Born in 1893 into a wealthy, religiously observant Jewish family in Manchester, Laski precociously shed his religion, eloped with a gentle suffragist eight years his senior, and failed his first-year's science courses at Oxford. But he then excelled as a scholarship student in historical and political subjects while participating secretly, if unsuccessfully, in at least one attempted act of terrorism against what he viewed as a repressive state. After graduation Laski abandoned his flirtation with syndicalist violence and proceeded to cram several lifetimes of work and endeavor into three decades, dying at age 57 in 1950 from lung disease, aggravated by his endemic chain smoking.

Laski's several compressed lifetimes included: a brilliant academic career beginning at McGill in 1914, moving to Harvard in 1916, where he attracted a lasting network of American friends, and finally to the London School of Economics in 1920, his academic home for the rest of his life; a teaching mastery over several generations of eager students drawn from many lands to become disciples; authorship of scores of books and hundreds of articles on jurisprudence, political theory, and praxis; public advocacy as a Fabian, journalist, and popular speaker for an array of progressive themes, including eugenics, female emancipation, decolonization, disarmament, human rights, worker advancement, and Zionism; and formal political activism in the British Labour Party as a Marxist theoretician and adviser to a skeptical Labour leadership, tireless campaigner on behalf of parliamentary candidates, participant on scores of committees, member of the party's National Executive Committee (1937–49), and chairman of the party in the crucial election year of 1945. He also made time for perennial lecturing tours in the United States: visits to India and the Soviet Union; the maintenance of an expansive, rich correspondence with British and overseas friends; constant hospitality for colleagues, politicians, and, above all, students; and parenting of a daughter, who responded to a secular, socialist upbringing by marrying a Tory classics don and converting to a devout Anglicanism.

With the centenary of his birth, Laski has received overdue treatment in two well-researched and well-written biographies. Michael Newman's *Harold Laski: A Political Biography* (1993) focused more directly on the development of Laski's political thought, from pluralism to Marxism and statist socialism, and his

critical dialectic with both communism and liberalism. Isaac Kramnick and Barry Sheerman offer more color and detail on Laski's life: the controversies, triumphs, and humiliations of this most prolific of modern British political intellectuals. Much in their book is new and instructive about the political culture of American and British academia and the ideological discourse generated by the postwar construction of the 1920s, the crisis of capitalism of the 1930s and the campaign for democratic socialism, the challenge of fascism and the coming of World War II, and then the politics of the Attlee government in fashioning the welfare state and joining the Americans in fighting the Cold War. By his writing, teaching, and political activism, Laski remained at the heart of the discourse on all the major political questions of his time; Max Beloff would term these decades "the Age of Laski." Beyond his role in the British Left, Laski also managed to attract the friendship, or at least the attention, of the politically influential and powerful in America and elsewhere: Felix Frankfurter, Oliver Wendell Holmes, Franklin Roosevelt, Jawaharlal Nehru, and even Joseph Stalin.

Kramnick and Sheerman leave their readers in awe at the scope and pace of Laski's intellectual and political engagements, yet their biography and Newman's candidly address the personal shortcomings and political contradictions that vitiated Laski's influence in the Labour Party and Labour governments. These problems left him on the periphery and disillusioned in the post-1945 period, quickly to be forgotten after his death. If we are shown how Laski projected himself onto center stage in the politics of his era, and if we see him playing his parts brilliantly, we are equally left with his own realization that in the end others had acted him off the stage, and that the dramas were still being scripted by "vast, impalpable forces."

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SUSAN KINGSLEY KENT. *Making Peace: The Reconstruction of Gender in Interwar Britain*. Princeton: Princeton University Press. 1993. Pp. x, 182. \$24.95.

In this book, Susan Kingsley Kent develops a theory of the causal relationship between the perception of gender relations during World War I and interwar feminism in Britain. To previous work on gender identity and cultural change by, among others, Paul Fussell, Samuel Hynes, Sandra Gilbert, and Sheila Jeffreys, Kent adds useful documentation and detail taken from a variety of published contemporary writing, periodicals, non-fiction, and experienced-based fiction.

In Kent's account, young men, during the initial stage of the war (1914–15), saw themselves—and were seen—as riding off to do battle in defense of home and nation; women saw themselves as without an active role, representing only that which was to be

protected. Many feminists immediately offered service in the conventional spheres of domesticity, serving mothers and children and looking to the nation's housekeeping. Later, the relatively few women who had front-line experience saw mutilation, pain, and death, and shared danger and emotional anesthesia with soldiers, coming to see them as fellow victims, no longer as heroes. But more commonly, between 1915 and 1918, men experiencing the horror of the front and women enjoying non-traditional jobs became hostile to each other, while sexual relations were charged with wartime urgency. Kent concludes that so serious was the tension between the sexes, and so close the discursive identification between the war and sex war, that feminists backed off from confrontation as the war ended, and "made peace" at the expense of the strong egalitarian principles of prewar feminists.

Kent's description of changing perceptions is valuable, but her argument is weakened by some vague use of terms, much generalization, and a base in assumptions that some feminist historians may see as questionable.

Much of feminism's struggle down the ages has had to be against the view that the nature of men and women is essentially different, the justification for separate spheres. But many feminists in the early twentieth century, and now, also have argued that women may have, by nurture if not by nature, a missing quality or experience to bring to public life. Ignoring the frequency with which this argument was used by prewar suffragists, Kent claims for them freedom from any taint of essentialism.

Kent falls into the common trap of conflating equality and sameness and—pace poststructuralism—compounds the problem by judging by words and not deeds. Was it really more "feminist" to seize the wartime opportunity to be like men, rather than trying to help women caught in the dislocation caused by the outbreak of war? Ideological approval seems to go, for example, to those involved in the Women's Volunteer Police Force, and to those helping recruit men for military service, and not to those who established workshops where women deprived of a living by the war crisis could work for pay, much less to any who believed the war made manifest the need for something new in the world of politics.

Central to Kent's thesis is the assertion that prewar suffragists saw themselves and were seen as engaged in "sex war," a term she never satisfactorily defines. Surprisingly, she cites George Dangerfield's now largely discredited view of prewar England and even gives credence to the likelihood of out-and-out war in the streets between men and women. Although some prewar rhetoric can be quoted in support of such a perception, it was closer to propaganda and metaphor than to reality. Again, Kent gives as the primary basis of the suffrage compromise of 1917 the "widely held assumption . . . that, prior to August 1914, a sex war had been raging throughout the land," which

would be resumed if no settlement were reached (p. 83), a statement that is even stranger in light of her categorization of prewar feminists as "overwhelmingly . . . bourgeois" (so presumably limited in number); she pays little attention to the growing labor base of the National Union of Women's Suffrage Societies (NUWSS). And it is a curious use of evidence to claim that when MPs welcomed the absence of a militant campaign in 1917 (as a mere handful did in their speeches), they were demonstrating "a near obsession with the possibility of another round of militancy" (p. 84). Reasons for accepting compromise were complex on both sides, and Kent fails to recognize that the NUWSS had already, long before the war, in effect abandoned its demand for suffrage "on the same terms as it is or may be given to men" (at one time a very limited demand) in favor of something much closer to "on the widest possible terms, but on any terms we can get, just to get a wedge in the door." Suffragists were already practiced in compromise.

The divisions within British feminism in the post-war period are well summarized; indeed, here Kent finally admits the possibility of more than one valid feminism, and no one can dispute the difficulty of adjudicating between the claims of "old" and "new" feminisms in this period, or deny the generally low profile of interwar feminism. We are, however, still left with fear of sex warfare as the main, almost the sole, reason for abandonment of Kent's artificially constructed pure feminism of the prewar era, with any rhetoric except that of pure "rights" regarded as tainted, and with motherhood as a "cultural cacophony" (p. 11) overwhelming egalitarian feminism.

Kent shows a good knowledge of her interesting material but does not recognize its limitations. It is unfortunate that her literary and polemical sources are not more consistently tested against other historical sources, and that she forces the whole into the mold of a theory, with little regard for the complexity of wartime experience and its effects.

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L. V. SCOTT. *Conscription and the Attlee Governments: The Politics and Policy of National Service, 1945–1951*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1993. Pp. ix, 304. \$53.00.

Britain has a longstanding tradition of relying on voluntary enlistment rather than conscription to fill the ranks of its armed forces in time of peace. The only major break in this approach to peacetime defense was National Service (1947–62). What L. V. Scott seeks to discover, through a step-by-step examination of the vast array of relevant government and Labour Party committee minutes and papers, is how this anomalous state of affairs came about under the Labour government of Clement Attlee. The result is a

monograph both massive in detail—sometimes to an almost overwhelming degree—and compelling in analysis.

As Scott ably demonstrates, the decision in 1946 to introduce a scheme of peacetime conscription was taken before the strategic needs and economic capabilities of postwar Britain had been properly understood. Once the true scale of military commitments and the full constraints on the economy became clear, a virtually insoluble dilemma arose for the Attlee government: the minimum number of conscripts and length of service considered necessary by the service ministries to carry out defense and foreign-policy objectives were almost invariably at odds with what ministers considered politically realistic and economic departments believed the country could possibly afford.

The result was a long series of cabinet committee and interdepartmental papers and meetings in the latter 1940s seeking to square the circle, punctuated by hurried shifts back and forth in the length of National Service or the size of the annual intake of new recruits brought on by immediate political crises or changes in the economic or international climate. In March 1947, for example, a revolt by backbench Labour MPs forced the government to reduce the proposed length of conscript service from eighteen to twelve months; a threat of resignation by the chiefs of staff in November 1948 drove it back up to eighteen months; and dangers raised by the outbreak of the Korean War in 1950 forced it up still further to twenty-four months.

It is easy enough with the advantage of hindsight to be critical of how the government went about planning conscription, but, as Scott himself admits with typical judiciousness, many of the international and fiscal problems Britain faced in subsequent years could not have been easily predicted in 1946. At the same time National Service, once established, rapidly became a problematic policy in its own right. Much of the deployment planning of the armed forces came to be bound up with conscription, and modifications to the system once it was in place, let alone the question of its abolition, involved immense practical difficulties.

Although Scott's book will prove rather tough going for readers unfamiliar with the structural intricacies of British defense-policy formulation, this excellent monograph is likely to stand as the definitive work on this particular subject. It remains to be seen whether Scott has ambitions to grapple at length with the other half of the story: how National Service came to be abandoned in the later 1950s.

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M. PERCEVAL-MAXWELL. *The Outbreak of the Irish Rebellion of 1641*. Buffalo, N.Y.: McGill-Queen's University Press. 1994. Pp. xv, 390. \$49.95.

The Rebellion of 1641 deserves to be regarded as a cardinal event of early modern Irish—and British—history. But its complexity and the fierce sectarian prejudices of most of its chroniclers have caused cautious modern scholars to avoid it. Seventeenth-century Protestants like Richard Baxter could believe that the rebellion led to the deaths of some 200,000 of their innocent coreligionists. Catholic nationalists then and since have usually minimized the violence, viewing it as incidental to the justifiable revolt of an invaded, colonized, and repressed people. Questions about the rebellion's causes, course, and level of bloodshed became mired in polemics, and the very term "rebellion" still invites controversy.

M. Perceval-Maxwell, the author of an excellent earlier study of seventeenth-century Scottish migration to Ulster (*The Scottish Migration to Ulster in the Reign of James I* [1973]) has entered this field bravely, and on the whole triumphantly. Building on the work of Aidan Clarke, Conrad Russell, and other late-twentieth-century historians of Scotland, England, and Ireland, Perceval-Maxwell sedulously explores the event and produces a coherent, palpable, intelligible account.

To compress and simplify his interpretation, Perceval-Maxwell gives us a rebellion with plotters and plots (some of which failed immediately) but that began quite specifically in the north and spread like a "brush fire," affecting various districts differently, the south and west figuring in events little and only rather late. The rebellion's wellsprings were many, but one of the satisfying aspects of Perceval-Maxwell's treatment is that he does not make the event seem inevitable but rather the result of a specific conjunction of circumstances. This is not a rebellion that had to happen because of English (and Scottish) Protestant oppression of Irish Catholics (however harsh that may have been). Rather, the event occurred fortuitously and was exacerbated in ways that no one could have foreseen.

Blame for the revolt is spread widely, almost diffused. Perceval-Maxwell exonerates Charles I of roguishly provoking the rebellion to serve his own dynastic ends. He exonerates aristocratic plotters from intending the bloody mayhem that ensued as their rude followers took justice into their own hands. He denies that the English Parliament cynically diverted funds raised for the repression of the rebellion in Ireland to its struggle against the king at home (although that is in fact what happened with the funds). And the earl of Strafford's harsh reign as lord deputy, on which the rebellion is sometimes blamed, emerges as one of the few things about which Catholics and Protestants could warmly agree: on the eve of the rebellion they worked concertedly to bring about his demise.

If these "usual suspects" are all excused, what caused the rebellion? To answer this question, Perceval-Maxwell takes a leaf from Conrad Russell, arguing that nothing was more destabilizing than the

example of the Scottish rebellion of 1637 and the wars to which it gave rise. Scottish Presbyterians, a powerful force and probably a majority in that kingdom, brought a hamstrung Charles I to his knees. The Catholics of Ireland, themselves still a powerful force and certainly a majority in their kingdom, saw a chance (and an even greater need) to do as much. Their failure to obtain the vaunted "Graces," and the twin menaces of a covenanted Scotland and a new anti-Catholic English Parliament, urged them (Old English and Gaelic alike) toward revolt.

None of this, according to Perceval-Maxwell, would have made much difference were it not for the presence of a Catholic Irish army, raised by Strafford to counter the rebellious Scots. Nipped in the bud as an anti-Scottish force by the Long Parliament, the Catholic Irish army was supposed to have been hustled off to Spain to be employed as a mercenary force in the closing stages of the Thirty Years' War. But Puritan objections to aiding a Catholic foreign power meant that the troops remained in Ireland, where they provided both encouragement and muscle for the rebellion that began in October 1641.

Not all of this is easy to follow in Perceval-Maxwell's painstakingly scholarly treatment, nor will everyone agree with his interpretation of particular points (such as the worth of the Earl of Antrim's "testimony" on the role of Charles I on pages 192–96). But this is an important book on a crucial subject, and future students will remain in Perceval-Maxwell's debt for many decades.

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THOMAS P. POWER. *Land, Politics and Society in Eighteenth-Century Tipperary*. New York: Clarendon Press of Oxford University Press. 1993. Pp. xiv, 376. \$59.00.

Although respecting the regional and particular aspects of County Tipperary in the eighteenth century, Thomas P. Power provides considerable insight into Ireland under the Protestant Ascendancy. The century witnessed the emergence of a highly diversified economy and a leveling of the landed elite at the top with the breakdown of the Ormond hegemony in the first half of the century, and the inclusion of entrenched graziers and head-tenants who took advantage of the expanding prosperity of the county, especially after mid-century. Most notable was the survival of a strong Catholic interest at the top of the social structure, reflected primarily in families who had strategically converted to Protestantism to evade penal law restrictions. They subsequently became established as the liberal Protestant party of the county, which relied on Catholic support. Even before Catholics were formally readmitted to the franchise in 1793, their political influence was strongly felt in a county where they outnumbered Protestants by more than ten to one. Aside from a temporary

eruption of ultra-Protestant sectarianism in the 1760s (set against the background of rural unrest and war with Catholic France), interdenominational relationships were remarkably placid. This was due to the integration of Catholics in the higher and middle ranks of the economic structure of the county before 1793, and afterward to the inclusion of Catholics in local government. The result, according to Power, was that Tipperary Catholics remained relatively immune to radicalization in the 1790s and escaped the trauma of rebellion in 1798.

This is all the more surprising because Tipperary was one of the hotbeds of agrarian unrest from the 1760s. In the strongest chapter of this excellent book, Power analyzes the several outbursts of rural revolt in the county, concluding that its general cause was rapid commercialization of agriculture combined with an expanding population, which created a high-rent, low-wage economy. Although class conflict was present in the disturbances, Power is careful to note the tensions within social formations. And an issue like tithe (the complexities of which he explains in the most lucid manner I have yet encountered) could galvanize classes across the social spectrum, from cottier to landowner, as it did in the Rightboy agitation of the 1780s.

If there is a weakness in the book, it concerns Power's account of the absence of radicalization in the 1790s. He seems to assume an economic-reductionist argument: no militant agrarian movement in the 1790s served as a catalyst for republican or Defender politicization. That may well be one factor, but Power fails to explain why the ideological appeal of the United Irishmen to the middle and artisanal classes of the towns and villages made so little headway.

Nevertheless, this meticulously researched book succeeds admirably not just as a county history but also as a general and highly accessible introduction into the complexities of the Irish economy and society during the crucial transitional period of the eighteenth century.

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JOANNA BOURKE. *Husbandry to Housewifery: Women, Economic Change, and Housework in Ireland, 1890–1914*. New York: Clarendon Press of Oxford University Press. 1993. Pp. x, 342. \$60.00.

Joanna Bourke's book is a valuable addition to the literature in Irish women's history and a contribution to Irish labor and economic history. The author traces the movement of women out of rural employment into unpaid housework. Her ideas are controversial and do not reflect the trend in European and Irish women's historiography, which have focused on women's efforts in the latter half of the nineteenth century to break from the domestic sphere and participate more fully in the public sphere. Bourke

shows that an increasing number of Irish rural women became unpaid housekeepers on the farms of their husbands, brothers, uncles, or other male kin. Moreover, she suggests they chose to do so. Demographic trends such as emigration, a low marriage rate, and celibacy (for almost a quarter of the adult population), combined with economic, agricultural, and technological changes in Irish farming, all contributed to this situation. The introduction of creamery cooperatives took butter making out of the hands of women on the farms and turned it over to male creamery employees. Likewise, the introduction of agricultural machinery transformed traditionally female tasks into male ones. Poultry-keeping, too, shifted toward being a male pursuit, although the change was less dramatic. Fewer women went into paid rural domestic service and instead remained at home as unpaid housekeepers. Bourke thoroughly documents this process with a wide variety of evidence. By doing so she provides a detailed picture of Irish rural life.

Governmental, agricultural, educational, religious, and charitable organizations all encouraged this trend, which conformed to traditional and official views of a woman's place and was thought to promote happier families, stem emigration, and provide male employment. Official attempts to establish home industries (for example, in embroidery, lace making, knitting, and crochet) on the farms were guided by the hopes that these jobs would keep women from being idle, provide them with some pin money, and establish a basis for later industrial development in Ireland. A successful cottage industry, however, never was established. Indeed, it failed largely because of superior foreign competition, lack of home demand, and, ultimately (here Bourke appears to agree uncritically with her seemingly biased and racist sources) because Irish women were unpunctual, uncreative, and lacked "energy and enterprise."

Bourke argues that Irish rural women voluntarily and happily went along with these changes to improve their "well-being." The alternatives were more bleak, such as being employed as underpaid servants in strangers' households, working in towns where there was less chance of finding marriage partners, or emigrating. She cites much statistical evidence to back up her claim that unpaid housework was increasingly replacing paid rural employment. Yet, although statistics can demonstrate trends, they do not necessarily provide insight into *mentalité*. One has to question her premise. To what extent was it a choice on the part of these women to improve their "well being," and to what extent were women making the best of difficult circumstances? Bourke avoids the pitfall made by many women's historians of inferring feminist tendencies where none existed. Nevertheless, to suggest what was forced on these women by economic, demographic, and technologi-

cal circumstances as choice seems to err on the other side.

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OREST RANUM. *The Fronde: A French Revolution, 1648–1652*. (Revolutions in the Modern World.) New York: W. W. Norton. 1993. Pp. 386. \$35.00.

Every twenty years there appears a fresh perspective on that most enigmatic of all French political upheavals: the judicial, popular, and princely uprisings of 1648–53 against the warfare state that Louis XIII and Richelieu bequeathed to the regent Anne of Austria. Orest Ranum provocatively subtitled his new study on the Fronde "a French Revolution." The author is not concerned with revolution in its traditional meanings, but rather what he repeatedly calls the "revolutionary moment." This account brings to the surface the underlying moralizing side of early modern French political culture in theater-like protests of word, plot, and deed: a stage within the broader stage of the Fronde. By placing his "revolutionary moments," beginning with the simple failure of subjects to pay wartime taxes and collusion by financial and judicial officials, at the center of the Fronde's stage, Ranum brilliantly captures the feelings of French subjects at their moment of greatest anger, using the richest archival evidence and surest analysis. There were broad-based and successful demonstrations by Parisians during the Day of Barricades against the royal arrest of a venerable judge, Pierre Broussel; popular rallies under the elm trees of Bordeaux against both the royal government and the local judges; and anonymous, often scatological pamphlets against the bankrupt state's semi-private creditors, called "snakes," and eventually against Anne's leading minister, Cardinal Mazarin. Long-winded orations, modeled on classical Roman republican rhetoric, against the corrupting influence of state-connected inordinate wealth came from the benches of the Parlement of Paris. And increasingly, as the initially powerful judicial and popular Frondes gave way to the princely Fronde, the debilitating posturing, plotting, and military maneuvering of great nobles upholding their own honor and power took center stage.

Ranum's Fronde is a highly imaginative and impressive application of several recent trends in historical analysis to political rebelliousness in two Frondeur centers, Paris and Bordeaux. His thick descriptions of material civilization enable the reader to hear, taste, touch, and smell agitated street life and cabaret political gossip. We gain new insight into old political thought through his deft cultural analysis of key words, ritual acts, and core beliefs of every segment of society. Above all, Ranum's approach liberates us from narrative history, which has traditionally seemed crucial to an understanding of any revolution, in order to pursue microhistory at its best. There is, of

necessity, a trade-off here. The sequence of events, functioning of political institutions, and practices such as venality of office become somewhat blurred as they enter, leave, and reenter the theater of the Fronde. But traditional history often sacrificed what Ranum has made intelligible and enjoyable. The sacrifice of scholarly footnotes is offset by a detailed index, brief historiographical essay, and listing of key secondary works as well as all of the author's archival sources, some of them newly uncovered. Finally, where Ranum's new history highlights the "great person" in history in insightful but brief character sketches, we will soon have Geoffrey Treasure's splendid, complementary book on that most elusive of all leading figures of the time, Cardinal Mazarin.

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DAVID A. BELL. *Lawyers and Citizens: The Making of a Political Elite in Old Regime France*. New York: Oxford University Press. 1994. Pp. xi, 280. \$35.00.

This is a rich and impressive book, in many ways a model for what a revised Ph.D. dissertation can and should be. David A. Bell recounts the history of the Parisian barristers or *avocats* (practicing lawyers, defined separately from the *magistrats*, or judges, who were professionally above, and from the *procureurs*, who handled the mechanical side of legal business who were below) from roughly 1660 to 1790. To do this he draws on a wide range of mostly hitherto unused archival sources; printed sources such as pamphlets, memoirs, and printed legal briefs; old and new relevant secondary works; recent scholarly work on eighteenth-century France as a whole; and some works of influential social scientists. The result is a book that not only tells a story in a way not told before but also serves as an important introduction for the nonspecialist to the new school of political history and the way it is being practiced for the study of eighteenth-century France. The tone, meanwhile, in an area marked by especially strong opinions and argumentation, is remarkably non-polemical, with the views of other historians presented, summarized, and integrated (or rejected) in a nuanced and always respectful way that is difficult to argue with.

Bell's two beginning background chapters describe first the world of the lawyers in general terms and then their building of what he sees as an independent profession in the second half of the seventeenth and the beginning of the eighteenth centuries. These are certainly helpful chapters, but he is most convincing and original—except for the very solid section in chapter 2 on the specific origins of the new Order of Barristers—in the final four of his six main chapters, which focus on the eighteenth century. Here he tells the story of the lawyers' activities and struggles, from their spirited resistance to the anti-Jansenist papal

bull *Unigenitus* of 1713 and their significant strike of 1730–31 and its aftermath, through to their divided reactions to the "Maupéou Revolution" of 1771–74, to their actions and continued divisions and clarification of positions in the pre-revolutionary period of 1787–89. The lawyers, he argues, were unique in the "absolutist" France of the early eighteenth century and after because of the special nature of their profession and, in particular, the freedom to speak out that the profession gave them. This enabled them to be at the forefront of the political evolution in France that was to lead to the French Revolution. Bell also shows how the French legal profession passed from being the tightly controlled and "establishment" (my term) order it was at the beginning of the century into the "raucous forum open to all manner of legal and political reformers" (p. 130) that he argues it had become in the last decades before the revolution.

This is a fine book that is also well written and enlivened with numerous examples of individual lawyers and their actions and writings.

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CYNTHIA A. BOUTON. *The Flour War: Gender, Class, and Community in Late Ancien Régime French Society*. University Park: Pennsylvania State University Press. 1993. Pp. xxvi, 307. Cloth \$45.00, paper \$18.95.

In this eminently readable volume, Cynthia A. Bouton provides a clear synthesis of previous work on subsistence riots, including that of Judith Miller, Guy Lemarchand, and Colin Lucas. Her extensive archival research provides the foundation for new perspectives on the *guerre des farines*, making of this relatively localized episode in 1775 a case study for discussing broadly significant questions of social and economic history. Her most fruitful contribution is to classify crowd behavior according to the type of community in which it occurred, defined by the relationship between community and market. She finds that in towns where grains were bought and sold primarily for a local market, more traditional forms of protest obtained, starting with appeals to local authorities to enforce a "moral economy." When these appeals were not heeded, rioters forced sales at a "just price."

More common were riots in towns that collected grain from the *pays de grande culture*, fed the large urban market of Paris, and relayed stocks through the conduits of a larger trade. In these centers, such as Magny-en-Vexin, Nanteuil, and Meaux, rioters more actively sought out stores of "hoarded" grain at the visible nodes of a developing commercial network, such as water-powered mills, storage depots, and large bakeries. An alternative to these preemptive attacks on the centers of commercial concentration of subsistence was the traditional *entrave* or interdiction of transport, as, for example, when a crowd comman-

deered barges of grain tied up at Pontoise. A fourth type of riot presaged the rural movements of the Great Fear in 1789: parties sought out the large producers in the countryside, suspecting them of hoarding large stocks in their barns for sale at ever higher famine prices. Such actions in fact constitute the great majority of all actions documented in the course of the *guerre des farines*.

Bouton has also documented some interesting differences in the rates of participation by gender and occupation among the different types of riots. The traditional market square riot was primarily the woman's province, whereas men took the lead in *entraves*. The search for stores in the countryside and in large commercial centers involved men and women alike. Bouton's claim that demography and the commercialization of the market tended to "feminize" men and raise their level of participation in actions directly motivated by hunger seems to stretch a point. The basic point her data show, however, is that the occupational vocabulary used to describe male rioters contained tell-tale qualifiers frequently indicating a status barely removed from being *sans état*, a result that significantly modifies George Rudé's thesis that the crowds contained no significantly "marginalized" actors (*The Crowd in History, 1730–1848* [1964] and articles on "*taxation populaire*" cited in Bouton's bibliography).

Bouton points to a promising area for future research in relating the Flour War to questions of "state-building" (p. 258), a topic that deserves to be considered in conjunction with "the famine-plot persuasion." As Steven Kaplan has noted (*The Famine Plot Persuasion in Eighteenth-Century France* [1982]), Charles-Clément François de Laverdy, Louis-Bénigne François Bertier de Sauvigny, and Joseph François Foullon, all at the levers of power when royal subsistence policies shifted, were put to death in the revolution for their alleged role in starving the people. Bertier, in particular, was marked for his role as intendant of the *généralité* of Paris, which made him responsible for provisioning the forces of order in times of dearth and trouble. Bouton's impressive command of the pertinent sources makes one hope that she will go on to explore the connections between the state mechanism of repression, direct state involvement in provisioning, and the fears at play on all sides. Why is it, for example, that "the Pontoise market drew the most intensive repression of any place that experienced the Flour War" (p. 101 n.)?

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PATRICE GUENIFFEY. *Le nombre et la raison: La Révolution française et les élections*. (Recherches d'histoire et des sciences sociales, number 58.) Paris: École des Hautes Études en Sciences Sociales. 1993. Pp. xi, 553.

Patrice Gueniffey's important and informative book fills a major gap in our knowledge of the French

Revolution. On the eve of the bicentennial, François Furet, a leading revisionist, commented that elections are the poor relative of revolutionary historiography. Historians had neglected elections in favor of popular movements, clubs, and the sections. Inspired by Furet, Gueniffey's book is a sophisticated revisionist work, the first of what promises to be a rich harvest.

Gueniffey aims to reorient our approach to revolutionary elections. Assuming that the vote leads logically to the result is an "illusion of transparency." To understand revolutionary elections, we must forget our assumptions about modern voting. Gueniffey applies to revolutionary elections Augustin Cochin's analysis of the elections to the Estates General, leading Furet to call him "our new Augustin Cochin" (p. ix). Unlike modern elections, there were no declared candidates, parties, platforms, or an electoral campaign. The vote occurred in a "political void." The voter was deprived of any meaningful choice because there was an unlimited "electoral offer" without real competition. A "militant minority," especially the Jacobins, dominated the voting. Elections from 1795 to 1797 belong to "post-revolutionary politics" because candidates were permitted and a means was provided to limit the electoral offer.

Gueniffey devotes his book to "the moment of the vote." Insisting on the uniformity of revolutionary elections, he rejects a chronological or regional approach. Because voting was the same everywhere, he studies a small, random sample of departments. Revolutionary electoral procedures were a "strange amalgam" of traditional and modern elements. The revolutionaries created a *censitaire* suffrage—quite low—based on indirect elections. Voting at the canton level and the generalized use of a written ballot were innovations, but votes by assembly vitiated individualization of the ballot. To prevent fraud, long and complicated procedures were adopted. Voting consisted of naming the best men among those eligible, not choosing among competing programs. The consequences were dispersed votes, low turnout, and the absence of meaningful elections.

The absence of declared candidates did not mean that there were none. Unofficial candidates were picked in secret meetings. Deals were made; cabals existed. Low turnout led to manipulation by a militant minority. The key to success was winning control of the bureau, thus occupying a strategic position to influence voting. Once the bureau was elected, the number of voters declined. Various strategies eliminated opponents, while the vote-tellers manipulated the ballots of the numerous illiterates. The characteristic trait of revolutionary elections is that the balance between political forces can only be discerned by the level of voter turnout, not the results. High abstention suggested a boycott by the opposition or a purge. Militants purged the assemblies by intimidation or using the loyalty oath. High turnout in 1793 meant mobilization by the Jacobins or the government.

Although Gueniffey claims to have discovered the

secret to understanding revolutionary elections, there are serious problems of interpretation and methodology. Foremost is the application of a preconceived interpretation. He only read the minutes of the primary assemblies of a small sample of departments for 1790–92. He favored those of 1792, which best suit his argument. Most of his data on voter turnout are taken from my work. He mainly read the minutes of the departmental electoral assemblies, located in Paris. He does not explain voter behavior by seeking correlations between voter turnout and variables such as clerical oath-taking or the network of Jacobin clubs.

Although there were irregularities, how widespread were they? Gueniffey admits that they are hard to find in the laconic minutes; their inclusion there would have invited annulling the election. Nonetheless, Furet states, “Gueniffey . . . mobilizes enough evidence from the era to show the universality of the phenomenon” (p. x). Hardly. Considering that there were over 5,000 primary assemblies, Gueniffey’s evidence is scanty. Having read thousands of minutes for 1790–99, I am struck by how scrupulous the assemblies were. Although voting declined after the election of the bureau, often the vote for electors at the end attracted the most voters. The number of bureau members chosen as electors was not inordinate.

Who were the militant minorities? Although there is no answer for 1790, starting in 1791 they were the Jacobins. Gueniffey admits, however, that the clubs were not very numerous in 1791; most were founded in 1793–94. While admitting that the Paris club never won an election, he argues that local clubs rarely lost. In fact, there was no centralized Jacobin machine. While local clubs definitely mobilized voters, I have been unable to find a strong correlation between turnout on the department level and the network of popular societies. I find the opposite in 1793.

Although Gueniffey’s book suffers from certain flaws, it is indispensable reading for anyone interested in the French Revolution or the creation of modern political culture. His argument that we must forget our assumptions about modern elections to understand those of revolutionary France is convincing, but his claim to have discovered the secret to understanding revolutionary elections is not.

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ANTHONY SUTCLIFFE. *Paris: An Architectural History*. New Haven: Yale University Press. 1993. Pp. x, 221. \$40.00.

There are architectural histories, and there are architectural histories. As Anthony Sutcliffe makes clear, his is not the usual. It is neither a guidebook aimed at the tourist nor the typical architectural history written by a specialist. Rather than a tale of great men and great buildings, his is an account of the evolution of architecture in Paris from how it appears, he maintains, to the person in the street. His thesis is that the

renowned beauty of Paris lies not in its monuments (which he says merge with their surroundings in ordinary vision), not in the work of individual architects who have risen above the norm by their talent and exceptional artistic integrity, but rather in the uniformity of its buildings, which gives the city a unique visual coherence. And this, he maintains, stems primarily from the strength of its classical tradition that dates back to the sixteenth century and remains a leveling force even today.

The thesis has problems. By focusing on the commonplace, the countless ordinary buildings that fill any city and constitute a backdrop for the exceptional, he underscores the importance of the latter—notable buildings such as the Notre Dame de Paris, Hotel de Sens, Perrault’s Louvre, Garnier’s Opera, the Eiffel Tower (which curiously he mentions only in passing)—by which Paris is typically defined. It is these that catch the eye of the casual passer-by, not the uniform multistoried apartment blocks against which they stand. Sutcliffe claims that architects in Paris tend to conform rather than innovate out of deference to the prevailing classical tradition. But how then does one explain the Perraults, Labrousse, Garniers, Eiffels, not to mention twentieth-century figures such as Le Corbusier or more recently Jean Nouvel, whose names are all but synonymous with thwarting the norm? The work of foreign architects in Paris—one thinks not just of Rogers & Piano, but of Frank Gehry, Richard Meier, and the Swiss (not French) architect Bernard Tschumi (whose deconstructivist Parc de la Villette Sutcliffe sees as respectful of Parisian classicism)—is all the more problematic.

Sutcliffe raises some good issues, such as the challenge Parisian architects faced in the sixteenth century transforming an imported Italian classicism into a national language, and their more recent efforts to reconcile modernism (which by definition is based on novelty, on inventing new architectural solutions to meet modern circumstances) and French tradition. After years of poring through the city’s archival resources, studying Paris’s by-laws and regulations, Sutcliffe knows its history well through its documentation, and argues persuasively that forces of ruling tastes, building codes, and cultural expectations have operated over the course of centuries to exert a certain leveling force over architecture in the city. His thesis, however, that the most powerful of these homogenizing forces is the French classical tradition, which has stifled creativity and compelled architects in Paris to conform rather than challenge, I find unconvincing. Much of this, however, is in the eye of the beholder: where Sutcliffe sees classicism, continuity, uniformity, and the same, others see transformation, change, differences, and the striking.

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MICHAEL PAUL DRISKEL. *As Befits a Legend: Building a Tomb for Napoleon, 1840–1861*. Kent, Ohio: Kent State University Press. 1993. Pp. xviii, 251.

In France the erection of a monument has often been the subject of fierce controversy: whether it should be built at all, where it should be placed, how it should be designed, and which artist should be assigned the work. Sometimes with a change in regime a monument again becomes the focal point of controversy, although as republicanism has become entrenched the French on the whole have come to accept the coexistence of royal, imperial, and republican monuments in their major cities. Controversies still rage, however, over the appropriateness and design of particular monuments. Both historians and art historians have come to realize that constructing a monument has to fit into a broad picture of the contemporary political, social, and artistic world. The planning of a tomb for Napoleon offers an ideal case study for the application of such an approach to history, and Michael Paul Driskel applies it in this book with impressive skill and artistry.

The French Revolution had left the country deeply divided into various hues of royalists, Bonapartists, and republicans. When Napoleon's remains were ceremoniously returned from Saint Helena to Paris in 1840, France was under the July Monarchy of Louis-Philippe, whose *Kunstpolitik* included exploitation of France's military victories as seen in the series of battle scenes he commissioned for Versailles. Napoleon's military exploits were the most outstanding, consequently a monument to him fit into this program. As a king, however, Louis-Philippe did not want to excite the workers in some of the crowded sections of Paris where Bonapartism still had great appeal. For this and other reasons the government rejected a variety of possible sites: the Vendôme Column, the Madeleine, the Arc de Triomphe de l'Etoile, the Basilica of Saint-Denis, the July Column, or a new site on the Champ de Mars or the hill at Chaillot.

Once the Church of the Invalides had been selected as the site, the battle over the design and direction of the construction began. This offers Driskel the opportunity to discuss what an architect is in the case of a project of great importance and social significance. He shows that the principal architect in this case, Ludovico-Tullis (Louis) Visconti, directed and coordinated a number of artists who were often in conflict. We are therefore given a fascinating glimpse of the *vie quotidienne* of a successful nineteenth-century artist and the amount of time he spent on non-aesthetic matters. Thus, Driskel's book contributes to the sociology of the architectural profession. At the same time it analyzes the discourse surrounding alternative plans for the tomb, the social meanings inscribed in the one actually constructed, how it was received by contemporaries, and why Louis Napoleon refused to inaugurate it for more than nine years, by which time Visconti was long deceased.

In addition to its analysis, this book is written with wit and beautifully illustrated. Unfortunately, the index is restricted to proper names.

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RAYMOND A. JONAS. *Industry and Politics in Rural France: Peasants of the Isère, 1870–1914*. Ithaca: Cornell University Press. 1994. Pp. xii, 219. \$39.95.

The central question of Raymond A. Jonas's book is political. He wants to know why the rural Isère voted socialist in the early 1900s. He finds the answer in the ruralization of the Lyon silk industry. The story Jonas tells will be familiar to historians of proto-industrialization. In the early nineteenth century, he argues, the Lyon silk manufacturers responded to the militancy of their urban labor force by putting work out into the countryside. Initially work was extended to male weavers who labored in their homes. In the 1870s rural mills began to overtake cottage industry. This process accelerated with the shift to mechanical looms in that same decade. As the building of rural mills and the process of mechanization continued, weaving became a female rather than a male occupation. Some women walked to work from farmsteads, others lived in company dormitories and returned to their homes only on weekends. By 1896, silk weaving employed one-ninth of the total labor force in the Isère (one out of four women and one out of eighteen men).

Although Jonas demonstrates that employment in the rural silk industry retarded the nineteenth-century rural exodus from many parts of the Isère, his major interest is in what he sees as the radicalizing rather than the preservative effects of the silk industry. The changes he describes are dramatic. In less than fifty years, a rural society in which risk-averse peasants engaged in subsistence farming and supplemented their earnings with male seasonal labor was transformed and radicalized. Men became responsible for both farms and hearths as their wives and daughters spent long days in the factories. Once away from home, women ceased to be dominated by men; took control of the public spaces in factory towns (spaces formerly dominated by men); engaged in strikes and labor demonstrations; and convinced their husbands and brothers to vote for the Socialist Party, which had supported their strike demands.

Unfortunately, there are problems with this picture that result from Jonas's sources and from his tendency to move beyond what he has documented. His portrayal of peasant farmers emerges not from a detailed study of the culture and agriculture of this region, but from outside observers (including historians) who depict rural France as poor, isolated, and economically and culturally backward in the eighteenth and nineteenth centuries. Many recent studies reveal a more dynamic, progressive, and flexible

peasant society, especially in regions of rural manufacturing like the Isère. Most seriously missing from the book is any interaction with the work of James Lehning, whose *Peasants of Marles* (1980) analyzed the impact of female employment in the silk ribbon-weaving industry on the neighboring department of the Loire. Unlike Jonas, who argues that rural industry destroyed peasant culture, Lehning made a convincing case for the adaptability of the peasant family and peasant culture in circumstances very similar to those in the Isère. The difference may reside not in the regions but in the demographic analysis that lay at the heart of Lehning's work and that is only sketchily attempted by Jonas.

Further problems arise with Jonas's assumptions about peasant and factory women that ignore the research of Natalie Zemon Davis, Olwen Hufton, Joan Scott, Louise Tilly, and the proto-industrial historians on women's roles in the peasant family economy and local marketplaces. His discussion of farm and factory work, women's family roles, and the relationship between wives' and husbands' occupations remains largely at the level of assumptions. And his argument that the male peasants of the Isère voted for the Socialist Party because it championed women workers is contradicted by his own evidence that local socialists appealed to men by defending private ownership of land, and by arguing that "women would best serve their own interests . . . by returning to the home, so that their jobs could be filled by men" (p. 161).

The Isère is an interesting and important region for historians to study. As Jonas reveals, it was the scene of female strikes and demonstrations; it was where the first French woman ran for the chamber of deputies on a Socialist platform; and it became a stronghold of the Socialist Party while many rural areas remained monarchist or conservatively republican. Earlier studies ignored the political dimension of proto-industrialization and Jonas is right to bring this issue into focus. What one wishes is that he had moved beyond the census summaries and early histories of this region to an analysis of the nominative censuses, *état civil*, and agricultural and manufacturing *statistiques* that would have yielded a more precise and complex picture of rural industrialization and politics in this region.

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CECILIA DUNOYER. *Marguerite Long: A Life in French Music, 1874–1966*. Bloomington: Indiana University Press. 1993. Pp. xvi, 240. \$27.95.

Cecilia Dunoyer describes coming upon the unsorted files of Marguerite Long in an unused bathroom of the Paris offices of the Concours Long-Thibaud, "piled up in the bathtub and balanced atop the bidet" (p. xiii). Dunoyer was given a chair and a table, and she set about patiently examining the life of one

of the most important French pianists of the twentieth century.

Long had the great good fortune of rising to prominence in a most fertile period for French music. The daughter of a railway employee in Nîmes, Long was discovered in 1888 by the National Inspector of Conservatories, who briskly informed her parents that they had no right to keep the child in the provinces and packed her off to Paris. She won the coveted Premier Prix of the Paris Conservatory in 1891 at age 16, earned a string of debuts with prominent Paris orchestras, and soon found herself within the exclusive circles of France's leading composers. "She is sensitive, she is intelligent," an early review reads. "With her, the piano disappears, the soul sings" (p. 24). Long introduced much of the piano music of Gabriel Fauré to the public, was a champion of Debussy, and premiered Ravel's G-Major Piano Concerto and *Le Tombeau de Couperin*. Dunoyer, herself a pianist, narrates the rise of this performer and pedagogue with deep sympathy, assessing Long's technique, canvassing her considerable repertoire, and summarizing the pedagogical approaches she perfected at the Paris Conservatoire and in her renowned master classes.

The documents Dunoyer has retrieved guide her account from achievement to achievement. Although the book occasionally reads as though the quotations are driving the prose, the dashed-off notes and unexcerpted letters are nonetheless engrossing. Vivid moments appear: the imperious Long trading sarcastic remarks with Vincent d'Indy, an aged Saint-Saëns kissing her with bits of cookie clinging to his mustache, Isaac Albaniz advising her to eat lots of steak before she premiers *Navarra*. Long's enduring legacy will surely be the establishment of France's first international music competition, the Concours International Long-Thibaud, which she and the violinist Jacques Thibaud inaugurated for pianists and violinists in 1949. Among the performers the competition has honored, and in certain cases launched, are Aldo Ciccolini, Paul Badura-Skoda, Yuri Boukoff, Pierre Barbizet, and Philippe Entremont; its juries have included Francis Poulenc, Arthur Honegger, Sergiu Celibidache, Artur Rubenstein, and Rudolf Firkušny. Dunoyer has done this vibrant competition a service by chronicling its early history.

In describing Long's personal relations, Dunoyer consistently reports her generosity and the genuine affection her admirers felt for her. The shadowy persistence of unspecified grudges, enemies, and resentments throughout the account, however, makes one wish Dunoyer had been a touch less reverent. We learn, for example, that Long engineered lavish dinner parties without paying for a single dish by exercising her charm over the telephone with friends, who invariably offered to bring their caviar, sorbets, and liqueurs. Long's pupils carried her mail, dialed the telephone for her, and were prepared to drive her to the dentist's or dressmaker's at a moment's notice.

She was especially harsh to her female students, and one forlorn letter from a dedicated assistant complains of receiving "not even a small mark of affection on a postcard" (p. 178). Dunoyer's admiration for Long is palpable, as was the esteem she earned from many of this century's greatest musicians. That this regard now and again seems too credulous may be the best expression of Long's commanding presence on and off the stage, a presence very convincingly drawn in this worthy biography.

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JOANNA E. ZIEGLER. *Sculpture of Compassion: The Pietà and the Beguines in the Southern Low Countries, c. 1300–c. 1600*. (Études d'histoire de l'art, number 6.) Brussels: Institut Historique Belge de Rome. 1992. Pp. 414; 119 plates. 1,900 F.

What to do with a category of numerous objects (sculpted images of the *Pietà*), created over an extended period, involving the repetition (with variations) of a common devotional theme? Many are now *membra disjecta*, detached from their original setting, audience, and function.

Earlier art historians might have arranged such objects according to formal criteria, seeking linear patterns of "development" and workshop filiation. Joanna E. Ziegler, however, having properly acknowledged her dependence on such connoisseurship, breaks from these well-worn paths, seeking instead to deal with the meaning of the object in relation to a clearly defined audience.

This is a book that will amply repay careful study, providing a valuable reading not just of the *Pietàs* or of late-medieval piety but also of the current state of art history. This is an impassioned book, driven by the belief that despite our separation from the works under consideration, there are, nevertheless, links between the way we respond—our "feelings"—and the response of contemporaries. Although I am in general agreement with this position, resultant tensions contribute to the weaknesses, as well as strengths, of the work.

The book's objectives are stated succinctly: "To pursue the exchange between the material (object) and the immaterial (feeling), and to explore the construction of emotions through art" (p. 15). Writing in the first person, the author declares her anthropological and feminist penchant: "I saw many old *Pietàs* still in use . . . It was the full-bodied, three-dimensional essence of the images, their sculpture-in-the-roundness, that struck me repeatedly. Believers came to pray to quasi-lifelike figures. What is more, most of the worshippers I saw were women" (p. 22).

The author's approach is focused through readings in Erwin Panofsky, Claude Lévi-Strauss, James Marrow, David Freedberg, and Hans Belting. She insists that "some experiences, like aesthetic emotion or

feeling, exist outside the social spheres of language and premeditated action" (p. 43).

It is somewhat disconcerting, therefore, to find oneself plunged directly (part 2) from this universality into the peculiar female micro-society of the Beguines. We must wait until a later chapter before the physical link between the surviving images and Beguine houses is explained. Thus, we are told, 25 percent of the *Pietàs* addressed women (how was this figured?), and of the 147 objects inventoried, nine had, at some time, a Beguine setting.

Despite the thin numbers that would have allowed Ziegler to document the link postulated in the book's subtitle, a strong case is constructed for the use of affective images by these semicloistered, mostly uneducated, urban women, often drawn from the poorer levels of society. The intense interest shown in the *vitae* in physical phenomena (blood, sweat, spittle, nail clippings, and so on) lead one to believe that close relationships would be formed with images used for contemplative purposes outside the liturgy. The Beguine woman might identify with the image of the Virgin Mary in the *Pietà*, particularly because many worked as nurses. In this way, it is postulated, a very close physical, even sexual relationship with Christ could be constructed.

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DANIEL ARASSE. *Vermeer: Faith in Painting*. Translated by TERRY GRABAR. Princeton: Princeton University Press. 1994. Pp. xiii, 136. \$39.50.

Johannes Vermeer has always proved an elusive artist, yet he has also held a special fascination. This new study of the painter, published in French with the more comprehensible title *L'Ambition de Vermeer*, makes considerable headway in outlining the reasons for both the elusiveness and the fascination. While drawing extensively on existing publications, Daniel Arasse nonetheless brings both a nicely calibrated visual sensitivity and a thoughtful analytical bent to the pictures themselves in order to lay out what he considers to have been a painting program by Vermeer.

No author writes in a vacuum, and Arasse has drawn heavily on the work of his predecessors. For the study of documents, he depends on the extensive archival findings of John Michael Montias (*Vermeer and His Milieu* [1989]), as well as earlier discoveries summarized in the works of Albert Blankert. For careful observations of the forms and themes of Vermeer, he owes much to, and sensitively builds on, the pioneering volume by Lawrence Gowing (*Vermeer* [1952]). And for a semiotic reading as well as contemporary perspective on optics, the arguments of Svetlana Alpers's *The Art of Describing* (1983) remains essential to this analysis.

But Arasse pushes our understanding of Vermeer further. He concentrates his attention on the two

most ambitious and comprehensive images: *The Allegory of Painting*, a work named and kept by the artist for himself; and *The Allegory of Faith*, centered on the Catholicism to which the painter converted when he married. In addition, Arasse's analysis attends to consistent yet neglected formal devices that form commonplaces in most of Vermeer's pictures: the painting-within-a-painting; the suppression of linear description in favor of blurring; and the placement of figures relative to the viewing angle and horizon of sight of the observer.

Some of his conclusions, confirming the observations of Gowing and Alpers, will not surprise the specialist. We learn that Vermeer makes use of such techniques and devices to problematize the process of seeing in relation to painting: "In Vermeer's eyes, the aim of painting was not to make its object known but to make the viewer witness to a presence" (p. 74). In Arasse's view, "the painting resists both visual and conceptual resolution" (p. 75). It may be a contemporary fashion to evoke the inevitable modernist shibboleth of ambiguity, but few artists have ever been so self-conscious in their presentation of painterly means or so defining of ambiguity to a post-Impressionist eye as Vermeer. And if along the way Arasse deconstructs traditional iconographic readings of these works as contradictory and unresolvable (while criticizing iconography as a method [p. 40]), then his examination of signification and how it is produced is calibrated to the relations that one finds in individual pictures as well as among them in the total *oeuvre* of the artist. And Arasse does succeed in accounting for the techniques and origins that evoke (along with Marcel Proust's Bergotte) our perceived "mystery" in Vermeer.

Primarily, this book is a close examination of images, and it is at its best when it attends to the consistent use of forms by Vermeer across his total output. There are useful comparisons to contemporary Dutch artists and their practices, but these appear chiefly to signal the distinction of Vermeer's accomplishment. The nonspecialist will need a highly developed interest in the artist in order to wade through the complex discussions of what might seem to be purely visual phenomena. Yet for an "appreciation"—in the traditional sense, yet applied with all of the sensibility that our post-ironic age has to offer—one might well wish to begin a careful study of Vermeer with this book.

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JOUKO HEIKKILÄ. *Kansallista luokkapoliittikkaa: Sosiaali-demokraatit ja Suomen autonomian puolustus 1905–1917* [Class Policy within the Nation: The Social Democrats and the Defence of Finnish Autonomy, 1905–1917]. Summary in English. (Historiallisia tutkimuksia, number 168.) Helsinki: Suomen Historiallinen Seura. 1993. Pp. 395.

During the first two decades of the twentieth century, the Russian government sought to integrate Finland with the rest of the empire by destroying the autonomy of the Finnish state. Jouko Heikkilä's work addresses the reactions of the Finnish Social Democrats to this process. He focuses on their attitudes toward nationalism and class politics during the decade between the revolutionary upheavals of 1905–07 and 1917, which in Finnish historiography has often been called the second phase of oppression.

Heikkilä argues that the Finnish Social Democrats developed a special concept of patriotism, different from that of the ruling classes in that it was tied to advancing the social, economic, and political interests and rights of the underprivileged. Although this may have distinguished the Social Democrats from the most conservative elements of the bourgeoisie, similar concerns were also voiced among the bourgeois Activists and Old Finns. The differences between parties often were mainly a matter of degree.

Heikkilä maintains that the Social Democrats regarded the defense of Finnish autonomy as part of the class struggle, since autonomy gave legitimate status to the party and the democratically elected national legislature. A desire to secure the party's gains, however, dictated caution to the party leadership in their dealings both with Russian revolutionaries and activist Finnish opponents of the tsarist regime. Although the party leadership shared the view that the tsarist regime was the common enemy, it was unwilling to take the same risks as the more revolutionary parties to undermine and destroy the regime.

Heikkilä has worked an often-plowed field. Among others, Osmo Jussila (*Nationalismi ja vallankumous venäläis-suomalaisissa suhteissa 1899–1914* [1979]), Antti Kujala (*Vallankumous ja kansallinen itsemääräysoikeus* [1989]), and Hannu Soikkanen (*Kohti kansanvaltaa*, vol. 1 [1975]) have covered much of the same ground. Therefore it is not surprising that relatively little is new in this book. Heikkilä tends to simplify his predecessors' arguments in order to emphasize the originality of his own work. But what he claims as new and original sometimes amounts to only an elaboration and amplification of previous research, expanded with partially new materials and close textual analyses.

Heikkilä's study is rather narrowly conceived. He does not dwell on the Russian developments, although he recognizes their decisive importance to the Finnish situation. What he does say about Russia lacks depth and nuance. He seems to accept uncritically that security interests alone influenced Russian policy toward Finland and that these interests not only necessitated Russia's policies of integration without compromise but were also in Russia's national interest. Both points are debatable. Heikkilä also does not much concern himself with the impact of Russian nationalism, although it not only helped to shape Russian policies toward Finland but was also an important contributing factor in the evolution of

Finnish nationalism and patriotism, including among the Social Democrats. Thus, he says little about an important causative and explanatory factor concerning his topic.

Concerning the Finnish working-class movement, Heikkilä has largely confined his research to the views of a relatively thin party elite. He analyzes their polemics and motives skillfully, and here lies the principal contribution of his work to scholarship.

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ANDREA HOFMEISTER-HUNGER. *Pressepolitik und Staatsreform: Die Institutionalisierung staatlicher Öffentlichkeitsarbeit bei Karl August von Hardenberg (1792–1822)*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 107.) Göttingen: Vandenhoeck und Ruprecht. 1994. Pp. 446. DM 92.

Andrea Hofmeister-Hunger's examination of Prussian state efforts to influence the press and public opinion during the Napoleonic period and after is a welcome addition to the literature. It will be useful mostly to historians already deeply involved in the study of European politics and culture in those years. Originally a Göttingen dissertation, this book shows Hofmeister-Hunger's significant talent in research and narration. Readers accustomed to typological and theoretical emphases in German theses may be surprised by the comparatively straightforward, chronological approach she takes. Readers will also be pleased by her clear presentation.

The boundaries set for this study are the years and places of Karl August von Hardenberg's service in the Prussian administration. From 1792 until 1803, he was chief minister of the margraviates of Ansbach and Bayreuth, southern outposts of the Hohenzollern state, encumbered by particularistic habits in the fragmented middle of the Holy Roman empire. The first chapter, occupying a third of the book, describes the challenges Hardenberg faced and the methods he used to reform administration in Ansbach-Bayreuth. He recruited publicists and journalists to help shape the local reception of new state policies. His collaborators also included a colorful mix of double agents, provocateurs, and poets. When both officials and writers were flexible, they formed mutually useful alliances, although usually short-lived ones.

A brief second chapter covers 1803–07, when Hardenberg was in and out of the royal cabinet in Berlin. The focus turns out to be less biographical than it seems at first, since Hofmeister-Hunger's attention now turns to *Öffentlichkeitsarbeit* (public-relations work) around Hardenberg rather than just by him. A bridge to what follows, this chapter shows how policies Hardenberg favored were stymied by King Frederick William III and his closest advisors. Hardenberg took a relaxed approach to the ambiguities involved

in reforming a traditional state during a revolutionary era. The king's view of the world was more rigid.

The third and fourth chapters follow Hardenberg's public politics from the time of Prussia's defeat by France in 1807 to the reestablishment of a European order in 1815. As first minister, Hardenberg shuttled between Berlin and battlefield headquarters, then between Berlin and Vienna. Away from Berlin he was more effective at influencing public opinion because he had greater opportunities for independent action. In the capital he struggled against hesitations about his methods in the court party.

Hardenberg thought that publishing acts of government was a positive opportunity for state propaganda, saw advantages in official newspapers, and preferred leniency toward journalists who criticized the state. But the polarization that grew during 1816–19 between a king who had promised a constitution and subjects who initially believed him hardly allowed for such temperateness. As Hofmeister-Hunger's fifth chapter shows, in the years leading up to his death in 1822, Hardenberg hewed, however reluctantly, to the increasingly hard line his monarch established toward opponents. In the end the Prussian government did institutionalize its press relations by establishing both a literary bureau and a state newspaper, but not in the reforming context Hardenberg had sought.

This book comments on and refines the large-scale works of scholars such as Reinhard Koselleck and Franz Schneider through a careful exposition of administrative policies and initiatives. As Hofmeister-Hunger freely admits, she does not address the public reaction to Hardenberg's efforts. Although she arguably overplays Hardenberg's historical importance, equating him with Klemens von Metternich and Otto von Bismarck, her focus serves well to illuminate the early nineteenth-century origins of Prussian efforts to mold public opinion through the press.

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FRIEDRICH JAEGER. *Bürgerliche Modernisierungskrise und historische Sinnbildung: Kulturgeschichte bei Droysen, Burckhardt und Max Weber*. (Bürgertum: Beiträge zur europäischen Gesellschaftsgeschichte, number 5.) Göttingen: Vandenhoeck und Ruprecht. 1994. Pp. 343. DM 64.

A good deal has been written on the dissolution of historicism since Carlo Antoni's *Dallo storicismo alla sociologia* (1940), including several studies in the past few years in Germany and Italy (by scholars such as Jörn Rüsen, Horst-Walter Blanke, Pietro Rossi, Giuseppe Cacciatore, Fulvio Tessoro, Annette Wittkau, Otto Gerhard Oexle). In all of them, history, or specifically the history of the West, is seen from Max Weber's perspective as a process of "rationalization" that empties the world of "meaning" (*Sinn*) and thus destroys the optimistic world view of the nineteenth-century *Bürgertum*. Friedrich Jaeger's book, originally

a doctoral dissertation, fits into this framework. He examines the "crisis of modernization" that he identifies with the *Bürgertum* through an analysis of the theoretical writings of three German thinkers—Johann Gustav Droysen, Jacob Burckhardt, and Max Weber—who have occupied a central place in all of the above-named studies.

For Jaeger, Droysen represents the best thought-out expression of the historicist world view (*Historismus*) characteristic of the German *Bildungsbürgertum*. Central to this view is the notion of *Geist*, with its focus on the intentions of people as agents of history and on social institutions as expressions of collective values and meaning. Jaeger's study seeks to show how this concept was transformed and "modernized" (p. 11) but not abandoned in the *Kulturgeschichte* of Burckhardt and Weber. Yet his sympathetic portrayal of Droysen contains elements of a devastating critique. This is valuable because a good deal of the recent literature has uncritically portrayed Droysen as the mastermind who placed modern historical scholarship on a firm theoretical basis. Jaeger stresses the extent to which Droysen uncritically accepted the complacent world view of his time. Like G. W. F. Hegel, Droysen saw history as a process in which freedom and reason had found their fulfillment in the *bürgerlich* society of nineteenth-century Europe. Jaeger could have made the point that Droysen's philosophy of history distorted his understanding of the past even more strongly had he examined not only Droysen's theoretical works but also his historical writings.

Jaeger points convincingly at a fundamental contradiction in Burckhardt's thought. Burckhardt was afraid of the development of a democratic mass society in a materialistic capitalist setting, yet at the same time he accepted fundamental assumptions of Social Darwinism. These included not only its stress on struggle (*Kampf*) and the "friend-foe relation" (*Freund-Feind Verhältnis*) (p. 108) but also its belief in the supremacy of Western civilization. Although on the one hand Burckhardt denied that there was a philosophy of history, on the other hand he shared Droysen's conviction that the history of the West consisted of the progressive unfolding of freedom, even if for him freedom also had its negative sides. In his view the culture of the West, based on the Hellenic notion of freedom, was not only superior to that of India or China (p. 103), but the future of the world lay in the hands of the "Christian Occident" (p. 175).

With Weber this process of disillusionment was carried one step further. For him as for Burckhardt the process of enlightenment, which originally freed people from "magical deterministic world images" through its inner "dialectic" (p. 182), led to new forms of irrationalism and determinism. In a world without objective meaning, values rest ultimately on the decisions of individuals. Jaeger questions this. Weber, the sociologist, in Jaeger's view turns out to be overly subjective in overlooking the role discourse

and communication play in establishing meaning (p. 265).

In the extensive literature on Droysen, Burckhardt, and Weber, Jaeger's primary contribution consists of the addition of a fresh critical dimension.

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STANLEY ZUCKER. *Kathinka Zitz-Halein and Female Civic Activism in Mid-Nineteenth-Century Germany*. Carbondale: Southern Illinois University Press. 1991. Pp. xii, 264. \$39.95.

As Stanley Zucker points out in the introduction to this biography of German author and political activist Kathinka Zitz-Halein (1801–77), the biographer of women must depart from the conventions of a genre that has hitherto been shaped by the life-patterns of "public men." Women's experience of the boundaries between "public" and "private" spheres, and of the pressures and rewards of political engagement, usually differs considerably from that of their male contemporaries.

In this biography of a figure who has previously been almost unknown to historians, Zucker emphasizes the relationship between public and private lives. Kathinka Halein, born into a prosperous, aristocratic family of Mainz, shared the lot of many privileged women of her time who were forced by family crises to earn a living. Halein turned to writing, as well as to other poorly paying jobs, at an early age. The breakdown of her marriage to lawyer Franz Zitz soon forced her into a protracted series of lawsuits for a financial settlement sufficient to support her as a separated woman. Zitz-Halein's development both as writer and as political activist was shaped by these private vicissitudes: her revolutionary zeal by her experience of marital oppression, and her literary career by the economic insecurity that often forced her to sacrifice quality to quantity.

Zucker's account of Zitz-Halein's public activities sheds light on a little-known aspect of the German revolution in 1848: the role of politically engaged women. Already deeply committed to liberalism, Zitz-Halein founded a women's organization in Mainz called the Humania Association in 1848. The organization promoted revolutionary causes and aided revolutionaries and their families. These activities brought her into contact with many prominent women, including Louise Otto, to whose journal, *Frauen-Zeitung*, she contributed some articles. Her membership in the German Catholic sect, which promoted both radical religious dissent and a new ethical basis for marriage and family, shaped her political ideas. After the suppression of the revolution, Zitz-Halein continued to support liberal causes through the literary works that are described in the final two chapters.

The author's success in uncovering and evaluating

a variety of source material on his subject is indeed impressive. The absence of references to some recent works on nineteenth-century German women is caused by the author's death in 1988; the book was published posthumously. Zucker's careful research and extensive knowledge of historical context make this a valuable work of history.

Throughout the book, Zucker expresses concern that Zitz-Halein was not "a feminist," at least as he defines that term. He is disturbed that, despite her vocal concern for gender inequality within marriage, she repudiated the most famous examples of female "emancipation" of her time and proclaimed domesticity as woman's highest calling. To criticize these views as insufficiently "feminist" strikes me as an anachronism; as Zucker himself points out, they were not much different from those of most feminists of her period. And the expectation that all women political activists will be feminists is too restrictive. No more than men have women throughout history confined their interest to any one set of issues. Indeed, especially during revolutionary periods, women's political activities usually support those of their male contemporaries. By transgressing the conventional limits of female behavior, these activities nonetheless stimulate new aspirations—usually disappointed—to respect and improved status. Zitz-Halein's story helps us to understand the variety and complexity of women's roles in revolutionary movements.

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FRIEDRICH LINGER. *Werner Sombart, 1863–1941: Eine Biographie*. Munich: C. H. Beck. 1994. Pp. 570. DM 98.

Werner Sombart's reputation as one of the most influential German social scientists in the early twentieth century was diminished after World War II because of the identification of his ideas with those of National Socialism. Recently there has been a renewed interest in Sombart, especially in Germany. The most important book in this new literature is that of Friedrich Lenger, who has written what will be the definitive biography of Sombart for some time to come. Based on extensive archival and published materials, Lenger's account, the first to examine Sombart's entire life, places him within three larger contexts: the culture of academia, the development of social science, and the main political configurations. In doing so, Lenger heeds Pierre Bourdieu's warning that a biography does not represent a simple and coherent expression of an "intention." He presents the changes, complexities, and inconsistencies of Sombart's life and writings, resulting in important correctives to the conventional interpretation of the national economist. For example, his discussion of Sombart's early career confirms the view that he moved from a favored position within the academic establishment to the status of outsider through his loose affiliation with the socialist movement and his

unconventional life style. Lenger notes, however, that despite this marginalization within the university itself Sombart became increasingly important within circles of the cultivated middle classes (whose supposed spokesmen were the same academics who castigated him) through his membership in important institutions such as the *Verein für Sozialpolitik* as well as through the popularity of his books and lectures. In addition, his standard of living, which partly resulted from his extra-academic popularity, exceeded that of academics superior to him in rank. When his status within the university community increased after 1918, his demand outside the university became somewhat diminished due to sociocultural changes in the republic.

Theoretically, Sombart moved from a concern for agricultural issues (much like his influential father) to become an adherent of industrial capitalism and a corresponding denigrator of precapitalist occupations that threatened Germany's cultural future. He then came increasingly to reject high capitalism as a mechanistically static system and instead promoted agricultural policy, calling for autarky. Politically, Sombart was at odds with the governments of the Kaiserreich, the Weimar Republic, and the Third Reich, as different as they were from one another.

Two areas where Lenger subscribes to the conventional interpretation but adds important qualifying levels of complexity are Sombart's socialism and anti-Semitism. In doing so, he corrects images fostered by Sombart himself at the end of his career.

Although Sombart had close ties to socialists (for example, with the editor Heinrich Braun) and was the first German academic to respectfully analyze Karl Marx's writings, he was never a member of the Social Democratic Party. He viewed the party as a vehicle for integrating the working class into the new capitalist order and as an alternative to revolutionary utopianism. Later in his career, he rejected both capitalism and socialism as elements of the same mechanistic system and briefly looked with sympathy on syndicalism, Bolshevism, and fascism as antidotes to the reification of the modern world.

Lenger, using correspondence and newspaper articles, demonstrates that Sombart's prewar writings and lectures on the Jews, which today are and should be seen as blatantly anti-Semitic, were viewed much more ambiguously at the time. He was actually praised by sections of the Jewish community (especially the Zionists) and castigated by some members of the anti-Semitic movement. Even when his writings became clearly anti-Semitic in the republic and Third Reich, he rejected the "biological" racism of the Nazis.

A short review cannot do justice to the complexity that Lenger portrays. His integration of Sombart's personal life, scientific development, and larger historical contexts, balancing appreciation with criticism, provides an exemplary model for intellectual

biography and is an important contribution to the history of German social science.

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JÜRGEN MICHAEL SCHULZ. *Kirche im Aufbruch: Das sozialpolitische Engagement der katholischen Presse Berlins im Wilhelminischen Deutschland*. (Beiträge zur Kommunikationsgeschichte, number 1.) Berlin: Walter de Gruyter. 1994. Pp. x, 376. DM 198.

This volume has all the merits and limitations of a minutely researched but narrowly conceived doctoral dissertation. Jürgen Michael Schulz's purpose is to establish the precise motives, ideals, goals, and methods of Catholic involvement in Wilhelmine Germany's social question during the last decade of the nineteenth century. To this end, he has examined in exquisite detail the reporting and editorial positions of the six newspapers that comprised the core of Berlin's Catholic press.

However much these Catholic papers differed in size, format, circulation, and importance, Schulz argues they were alike in loudly voicing Catholic interest in the social question. That interest, he concludes, stemmed from a combination of genuine concern regarding social distress in Germany's industrial towns, the fear of Social-Democratic competition after the government's antisocialist legislation expired in 1890, the need to find an effective means by which to reforge Catholic unity in the aftermath of the *Kulturkampf*, and the desire to present the church and its adherents as a mainstream political and social movement.

Not only did Berlin's Catholic press call attention to the plight of the working classes, Schulz insists, but it also recommended specific remedies for the redress of their grievances. Some newspapers offered the prospect of social reform through political collaboration across sectarian boundaries. Others emphasized the need for interconfessional trade unions and collective action. Still others advocated ordinary Christian charity. All stressed the need for spiritual renewal as a precondition for economic and social reform.

Many of these recommendations, Schulz admits, were naïve and impractical. The church hierarchy's insistence on patrimonial control over Catholic workers, to cite only one example, was doomed from the start because of an acute shortage of clergy and insufficient numbers of churches in the German capital. For all that, however, Catholic involvement in the social question led to the creation of social institutions like the *Volksverein* that compensated for the absence of any kind of formal organization in the Catholic Center Party. This involvement also served to reintegrate Catholicism's ranks and made it once again—at least until the turn of the century, when disputes over trade-union organizational forms again disrupted that cohesion—into a coherent movement.

If this close examination of Berlin's denominational press enables Schulz to delineate the competing strands underlying the broad pattern of Catholic social concern during the 1890s, he is less successful in explaining how editors, journalists, and publishers perceived important and specific municipal or state social interventions like emergency public works projects, traditional poor relief, or unemployment insurance. Nor does this approach, at least as employed here, tell us what Catholics thought about gender and family issues, the innovative social-hygiene programs then coming into vogue, or even the emergence of a group of social professionals who administered these programs in Wilhelmine Germany.

Much has been written on this period and problem, and Schulz's conclusions, if not his emphasis on the denominational press, will be familiar to readers acquainted with that literature. What will surprise them, however, is the absence of communication theory or any kind of theoretical underpinning to explain the role and significance of the newspaper press. Schulz's uncritical reliance on the newspaper as an evidential source is reminiscent of what passed for scholarship in some historical writing two or more generations ago. Surprising, too, is his preoccupation with Berlin and its relatively small Catholic population—about 135,000 or so in 1890—without any systematic analysis of that community's social composition or comparison with other Catholic areas and regions in Wilhelmine Germany.

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THOMAS J. SAUNDERS. *Hollywood in Berlin: American Cinema and Weimar Germany*. (Weimar and Now: German Cultural Criticism, number 6.) Berkeley and Los Angeles: University of California Press. 1994. Pp. x, 332. \$40.00.

Calls to widen the perspective on Weimar culture are being heeded. The few luminaries who used to dominate the historiography—Bertolt Brecht, Walter Gropius, Käthe Kollwitz—still draw attention, but not at the cost of the larger institutions and industries that housed or produced Germany's arts and amusements during this era. The cinema is a prime subject for this form of analysis.

Thomas J. Saunders has not written yet another flamboyant study of Weimar's legendary films and directors. Rather, his book is an intelligent and continually rewarding history of the German "film community" (p. 18). At issue is the German reaction to America's cinematic preeminence.

True, the "film community" is a far-flung abstraction; everyone from the actors and actresses to the company heads, from the theater owners to the critics, belonged. By concentrating mainly on the last of these, Saunders gains a measure of coherence for

his work. Yet the critics themselves present variety. Not only did opinions differ but so, too, did the basic relationships with the film industry. One sort of critic published in trade magazines, a second published in newspapers, and a third wrote for journals concerned with the arts. At times the first accused the others of ignorance of the medium, while the artistically inclined prized their sophistication as well as their freedom from the profit motive; they tended to see trade reviews as mere advertising. Still, amid diversity there were common patterns. Critics may have assessed the Americans differently, but they tended in general to acknowledge American success and to identify American cinema as the point of comparison for domestic production. Politics, too, divided the critics, yet Saunders concludes that "political allegiance [was not] . . . the key to the bulk of critical commentary" (p. 49).

Germans began to anticipate the American influence shortly after World War I, although it was not until 1921 that wartime import restrictions were lifted and foreign films appeared in large numbers. Inflation slowed imports in 1922–23. Thereafter, and until the "sound revolution" (p. 221) of about 1930, American films were a substantial part of the German market.

That Germany merited its own cinema, famed and profitable at home and abroad, was a premise common to German critics. Hollywood dominated the Germans—through film imports as well as through investments, a variety of German partnerships and subsidiaries, and the employment of German performers and directors in America. Nevertheless, Berlin aspired to be Hollywood's competitor. Regarding German exports, high hopes had been encouraged early in the decade by reports (probably exaggerated) of the positive reception of a few German films, such as *Das Cabinet des Dr. Caligari*, by American audiences. These hopes faded quickly. By 1923 most observers conceded that the Americans had never lost the upper hand with their own public and were gaining it in Germany. Yet "the appeal of the American market was so powerful that German producers worked with at least one eye on [its] possibilities" (p. 64).

Certain critics railed against Hollywood sensationalism, moralization, and happy endings: Hollywood pandered to the masses. Some saw American hegemony as a foregone conclusion, owing to American capital. The opposition to Hollywood reached a crescendo in 1924, when the sheer number of American films, many of poor quality, antagonized critical and public opinion alike. Nonetheless, "Hollywood became a fixture in Weimar film culture" (p. 119). Throughout the 1920s there were observers who looked past their aversion to specific films and framed a critical discussion of the American achievement as a whole. Hollywood was scanned for lessons. Without ignoring the American business practices and filmic techniques, these critics focused on the nature of the new medium and its connection with democracy.

Hollywood, they contended, was solving the riddle of film. More radically than the Germans, American film makers shunned the artistic traditions of literature and the theater, recognized the modern, industrial character of film, and formed an artistic product that had the qualities of industrial life: spontaneity, simplicity, speed. The Americans also had drawn the right conclusions about art in a democratic society: popularity was not the dead weight that German recalcitrants assumed it to be; rather, it was the necessary attribute of any form suited to a mass, democratic age. Therefore, a German film identity would be secured not by rejecting the American experience but by absorbing it.

Saunders has laid a basis for further study: the film makers' answer to the critics. In the meantime, we have a fine book that broadens our understanding of Weimar film and of internationalism as an aspect of Weimar culture.

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SYLVELIN WISSMANN. *Es war eben unsere Schulzeit: Das Bremer Volksschulwesen unter dem Nationalsozialismus*. (Veröffentlichungen aus dem Staatsarchiv der Freien Hansestadt Bremen, number 58.) Bremen: Staatsarchiv Bremen. 1993. Pp. 398. DM 54.

Education in post-1870 Germany was primarily the responsibility of regional and local authorities (and it still is). Only at the outset of World War II did the Nazi regime attempt to unify the nation's school systems in the name of ideological conformity, but that effort did not survive military defeat. This decentralization justifies Sylvelin Wissmann's decision to focus her now-published dissertation on elementary schooling during the Third Reich on the single city-state of Bremen. She rightly maintains that an in-depth examination of its experience will yield valid general insights about German schools at that level (the first eight years of instruction).

Wissmann's detailed study is organized not only chronologically (with September 1939 providing a basic dividing line) but also thematically: she analyzes both "the school within the Nazi state" and "the Nazi state in the school." After a somewhat cursory glance at Bremen's educational structure during the Weimar Republic, which identifies several left-wing pedagogical progressives who later accommodated themselves more or less enthusiastically to National Socialist objectives, she assesses the impact of Hitler's seizure of power. She finds, on the surface, greater continuity than discontinuity: the city's handful of experimental schools reverted to normal status, coeducation was abolished, and in all forty-four teachers lost their jobs (but only a few were reformers and most were subsequently rehired, at the latest under wartime conditions of acute personnel shortages). More significantly, what Wissmann calls the "meta-curriculum" of

the Nazis—a program “of character building by imparting a certain pattern of action and behavior” corresponding to the party’s ideology (p. 339)—was inculcated through incessant repetition, emotional (rather than rational) appeal, and an all-encompassing presence in the schools of the regime’s representatives (in such forms as the Hitler Youth and the armed forces).

The result was the almost complete absence of any type of dissent, let alone opposition, by pupils or indeed teachers faced with even the most heinous measures of the dictatorship. On the basis of an exhaustive search of school records and comprehensive interviews with former members of both groups, Wissmann concludes that by 1939 only two boys or girls at any one institution were not enrolled in a Nazi youth organization, “and that estimate is probably too high” (p. 177). Worse, two teachers actively participated in the pogrom of November 1938: one helped set fire to a synagogue and the next day triumphantly waved stolen torah scrolls before his colleagues (pp. 173–74). In the sole documented instance of open resistance by anyone connected with Bremen’s *Volksschulen*, four Lutheran female teachers prayed in public together with a group of Jewish Christians on the night before their deportation eastwards: “A similarly demonstrative act on behalf of Jews of the Mosaic faith is unknown” (p. 287).

In light of the frequently observed tendency among her living sources even a half-century afterwards to reverse cause and effect (many still spoke of the war that was “forced upon” Germany), Wissmann’s final summation seems rather generous. She maintains that a considerable number of teachers managed, given existing limitations, to uphold traditional humanistic ideals and at least to moderate the most serious effects of Nazi decrees on their charges. If her book also sometimes contains excessive detail, such as the function of janitors (*Hausmeister*) and their wives, it also includes important information about the activities of the Air Raid Protection League in the prewar militarization of the schools, the insidious nazification of everyday speech and terminology among pupils and their mentors alike, and the remarkably efficient evacuation of Bremen’s school population to less endangered areas of the country after mid-1943. This account is both valuable to the nonspecialist and broadly applicable to educational circumstances throughout Nazi Germany.

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JOANNE M. FERRARO. *Family and Public Life in Brescia, 1580–1650: The Foundations of Power in the Venetian State*. (Cambridge Studies in Italian History and Culture.) New York: Cambridge University Press. 1993. Pp. xvii, 258. \$59.95.

Our understanding of the social and economic history of early modern Italy has greatly advanced re-

cently. Joanne M. Ferraro’s book on Brescia represents a solid contribution to this broadening of our knowledge of the social and economic processes at work in the early modern Italian states. Ferraro focuses on one of the wealthiest cities under Venetian dominion, and in particular on a period that saw demographic and economic expansion followed by plague and economic crisis. At the center of the book is the Brescian oligarchy and its relationship both with the rest of the urban population and with the Venetian government. This allows Ferraro to address two related themes that have interested historians recently, and indeed she provides a very clear historiographical background on her topic.

The first theme is the “aristocratization” of the Italian urban elite in the post-Renaissance period, as families of different origins but united by their domination of urban institutions managed to make city government more exclusive and gradually became an amalgamated elite whose wealth was based on a landed patrimony and who exhibited an aristocratic life style. The second theme is the relationship between the desire of the central authorities of the regional state for centralization or at least greater control of provincial life and the continued strength of local institutions and elites. In Brescia, as in many other Italian cities, local autonomy went hand in hand with the growing power and wealth of a narrowing urban elite.

The book is based on a variety of sources, from fiscal and administrative Brescian sources to Venetian documents, from family papers to statutes. The results of this research are presented clearly and effectively in a series of well-conceived chapters. After an introductory chapter describing the structural framework (geography, history, administration, economy), the book is divided into two main parts. The central part deals with the process of “aristocratization,” by analyzing first the concentration of power and wealth in the hands of relatively few families within the coun-tilar oligarchy of Brescia, and then the preservation and transmission of that wealth and power through marriage and patrimonial strategies. In this part Ferraro effectively blends her general study of the Brescian elite with revealing examples concerning both subsets of the elite and specific families and individuals.

The last part of the book turns to the interaction between Venice and Brescia. After a discussion of factions in Brescia, there follows a chapter on the benefits civic offices offered to the local elite, such as the administration of local charities, taxation, and grain provisions. The confusing borders between public and private patrimonies, and between the interests of the city government and those of the citizens that ran it, found here wide scope for corruption and inefficient administration. A final chapter analyzes the demand in 1644–45 by wealthy Brescians excluded from political power for an opening up of the oligarchy and the Venetian response. The Vene-

tian government, Ferraro concludes, although often unhappy with the elite's administration, ultimately collaborated with the local oligarchy and helped maintain its grip on power, out of concern for social order, its insufficient means of pressure, and its own commitment to aristocratic rule.

The book does not offer as strong a discussion of the relationship between the city and the surrounding countryside as its own introduction seems to promise. Although there are frequent comparisons to Tuscany, Lombardy, and the Papal States, the book might have benefited from consideration of other parts of Italy, namely the southern kingdoms (which are almost totally ignored) or Piedmont (surprisingly, Stuart Woolf's old but classic work on the Piedmontese nobility is not mentioned). But this does not detract from Ferraro's interesting and well-written book, which will be of use to all those interested in early modern Italy and in early modern European society.

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LUCETTE VALENSI. *The Birth of the Despot: Venice and the Sublime Porte*. Translated by ARTHUR DENNER. Ithaca: Cornell University Press. 1993. Pp. 119. \$22.50.

Lucette Valensi purports to trace the change in Venetian attitudes toward the Turks during the later sixteenth and early seventeenth century in this short book. Her sources are the reports by Venice's ambassadors and envoys to the sultans during this period, read before the Senate at the conclusion of their missions. Valensi's thesis can be stated briefly: Venetians, like other Europeans, initially admired the order of the Turkish state, administration, and army, despite the fear that the seemingly relentless Turkish advances engendered. The Ottoman sultans were occasionally even likened to Roman emperors because of their success as conquerors who knew how to build a well-organized empire. Toward the end of the sixteenth century Venetian perceptions of the Turks changed. The Ottoman state was more and more frequently seen as an empire of slaves, its rulers despots and tyrants who were contrasted unfavorably with legitimate European governments. The seeds of Montesquieu's influential view of Oriental despotism were planted.

It would be a pleasure to report that Valensi offers fresh perspectives on an established topic. But the first reaction of a reader is puzzlement. Why was this slight essay published as a book? Anyone who has even glanced at Paolo Preto's magisterial *Venezia e i Turchi* (1975) will find nothing new here save some trendy words. As Preto has shown, ambivalence toward the Turks has a long history in Venetian thought. In Venice, admiration of Turkish martial valor and of the Ottoman empire coexisted for centuries with revulsion toward many features of Turkish culture, religion, and everyday life.

Valensi's treatment of this theme has both major

and minor problems. Most serious is the lack of a convincing explanation for the change in Venetian attitudes. A lengthy discussion of Jean Bodin's ideas remains quite inconclusive in this context. The statement that "Counter-Reformation culture" was a "threat to Venetian independence" (p. 91) leaves one mystified. Why "culture"? Antonio Vivaldi's oratorio "Juditha triumphans" is awkwardly introduced as a metaphor at the beginning of the book, only to disappear without a definite connection with the text. Finally, it is unclear whether the author or translator should be credited with a real howler: "Erode, il quale despoticamente governava" ["Herod, who ruled in a despotical fashion"] is translated as "That which is governed despotically is undermined" (p. 92).

Short books on major subjects can be a pleasure to read. This one is not.

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PERRY R. WILLSON. *The Clockwork Factory: Women and Work in Fascist Italy*. New York: Clarendon Press of Oxford University Press. 1993. Pp. 291. \$55.00.

Histories of women's experience under Italian fascism have most often focused on institutions, prescriptive propaganda, and available statistics such as censuses. Perry R. Willson changes all that, for she draws on an engineering company's newspaper and personnel records as well as interviews with former workers in Sesto San Giovanni, north of Milan.

In the first decades of this century, the heavy engineering and rubber firms that had been so important in Milan's first industrialization, seeking space for newly expanded and redesigned factories, moved north. Magneti Marelli, a new firm that made radios, batteries, and the like was established in 1919, also in Sesto; by World War II it was one of Italy's largest and most modern businesses.

Willson opens with several excellent chapters that trace the firm's history, especially its introduction and practice of both Taylorist and Fordist organizational techniques: time-motion studies and assembly line production. Here she introduces the owners and management of the company as individuals, whose careers and eventual disgrace or rehabilitation after World War II form a continuing thread in her story. These men (there was only one high-ranking woman, the tough—but always ready to support "her" workers—supervisor of the all-female winding-room, who also gets extended treatment) not only preached and implemented scientific management but they also attended to the "human factor," seeking to build consensus by providing "benefits" like a school for male apprentices, courses in domestic science, day care, and a nursing room for women workers, and by involving employees in a safety committee. All of these served the company's interests, of course, and were organized in an undemocratic and hierarchical

fashion; the bonus incentive pay system was so complex that workers never knew what they had earned. Former workers reported that they had tried to "beat the clock" by slowing down their pace when they were timed; their wages were no lower than those in other engineering firms, and female wages were better than those in textiles, which was the best alternative for women.

Magneti Marelli had a much higher proportion of women workers than did Sesto's heavy engineering firms; indeed, Willson shows that despite fascists' extolling of women as mothers and homebodies, women throughout Italy continued to hold jobs during the fascist period because their wages were needed by their families. Although their tasks were monotonous and repetitive, and they were strictly disciplined and paid less than any male worker, Magneti Marelli female workers took pride in their work; they had internalized the firm's values. Women's on-the-job relations were often marred, however, by jealousy.

Moreover, all women workers carried a double burden. Whether they lived with their parents or with a husband and children, they had heavy responsibilities in the home. Even their "leisure" was spent knitting, mending, or embroidering; her female informants told Willson that the company's fascist Dopolavoro leisure program did not involve women much, or at all. In the resistance period, those women who contributed to the cause did so primarily in a nurturing role by preparing food, knitting, or making clothing.

Overall, there were elements of both emancipation and oppression in these women's lives; Willson warns, however, that their jobs and workplace were exceptional in Italy, where domestic service, laundry work, and the textile and garment industries were much more likely female employment. Willson has made the most of her excellent sources; her study is an outstanding example of social and business history and gender analysis.

LOUISE A. TILLY
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H. JAMES BURGWIN. *The Legend of the Mutilated Victory: Italy, the Great War, and the Paris Peace Conference, 1915-1919*. (Contributions to the Study of World History, number 38.) Westport, Conn.: Greenwood. 1993. Pp. xix, 343. \$55.00.

H. James Burgwyn describes Italy's participation in the Entente's war with Italy's former allies, Germany and Austria-Hungary, a wartime arrangement Burgwyn calls a "*mésalliance*" (p. 2). A careful reading will show this to be a euphemism, there never having been a formal treaty binding Italy to the Entente.

In Burgwyn's account, Italy's war aims were clearly at odds with those of the Entente. As a result, when France and Britain, its surviving members, redrew the Euro-Mediterranean map, Italy's wartime leaders, Vit-

torio Orlando and Sidney Sonnino, were treated as interlopers. Italy had plotted its own course, suffering the consequences, both in the Adriatic-Balkan sphere and in the booty of the defunct Ottoman empire.

The postwar's "Mutilated Victory" slogan expressed widespread hatred of France and Britain, which had expanded their empires, robbing Italy (so it was widely believed) of its just share of the spoils of a common victory. Because the slogan gained wide currency, gathering together the poet Gabriele D'Annunzio, Benito Mussolini, the journalist Luigi Albertini, and the historian and later antifascist exile Gaetano Salvemini, to name but a few of the principals, Burgwyn wonders whether sentiments so intense and so diffuse had given Italian politics a "Germanic orientation," culminating in Italy's war of "vengeance," the June 1940 invasion of France (pp. 318-20).

The most prominent target of these defenders of the war's necessity, known then and ever since as the "interventionists," was Giovanni Giolitti, protagonist of prewar Italian politics and leader of the parliamentary majority that had hoped to preserve the neutrality Italy enjoyed from August 1914 to May 1915 within the old Triple Alliance. That Giolitti's fifth and last ministry (1920-21) had negotiated the Rapallo Treaty with the rival state of Yugoslavia was but one case of betrayal of the victory, of a piece with his efforts to prevent war in the first place.

Giolitti and the "neutralists" are marginal to Burgwyn's book. His focus is on those "interventionists" whom he labels "democratic," heirs to Mazzinian idealism; or on such liberals as "the great Luigi Albertini" (p. 11), advocates of war whose motives and acts he emphatically distinguishes from such of their "interventionist" comrades as the National-Imperialists or the renegade socialist Mussolini.

The bulk of Burgwyn's book, however, is a well-organized, clearly written record of the tensions of this "*mésalliance*," a handy guide for specialists as well as generalists through a maze of shifting and evolving war aims. Included are some helpful maps.

Being primarily a diplomatic-military chronicle, Burgwyn's major interest on the Italian side is Sonnino, foreign minister since October 1914, who planned what he called "our war"; but also Luigi Cadorna, chief of general staff from June 1914 until the Caporetto disaster in 1917 ended his career. If Italy and the Entente were at odds, so were Italy's political and military leaders. In a chapter dedicated to these "Two Strong-Willed Men" (pp. 51-70), Burgwyn shows that, although both predicted a short war, Sonnino fought for limited strategic goals, while Cadorna contemplated a grand Napoleonic-style attack into the heart of the Habsburg monarchy. Burgwyn has no quarrel with the established view that neither of the two ever wished to take on Germany, even though the language of Italy's talks with the Entente seemed to require this in the event Italy did decide to go to war.

Of Sonnino's policy Burgwyn writes that "If single-minded and selfish, Italian diplomacy was, nonetheless, marked by an essential consistency grounded on the Bismarckian principles that diplomacy should reflect the equilibrium of power, far from the purview of public opinion and at the exclusion of idealism and abstract justice. This, for good or evil, was normal behavior for European states in the prewar decade." That Sonnino's dickerings with Berlin and Vienna for the preservation of neutrality was a "charade," designed to outwit the parliamentary majority (p. 30), Burgwyn takes to be an aspect of such *realpolitik*.

Burgwyn's book is dedicated to another "democrat" and "interventionist," Woodrow Wilson, who first demonstrated that "Aggression and militarism were no longer considered acceptable among civilized states" (p. 316). He ponders whether Italy might have moved toward freedom rather than tyranny had a "democratic interventionist" and not Mussolini risen out of the war to power.

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PIOTR S. WANDYCZ. *The Price of Freedom: A History of East Central Europe from the Middle Ages to the Present*. Paperback edition. New York: Routledge. 1993. Pp. xv, 330. \$16.95.

The need to understand East Central Europe after the collapse of communism makes the appearance of this fine book doubly welcome. Piotr S. Wandycz, known for his distinguished work on interwar Polish diplomatic history, provides a balanced introduction to the region; only the last chapter discusses communism, including a perceptive view of "Post-Communist East Central Europe."

Wandycz defines the region in political rather than geographical terms, a decision that blurs the regional focus somewhat. The book's title marks its overriding theme; it refers to the difficulties of establishing and maintaining national independence over the centuries. Wandycz provides an excellent comparative study of the political, economic, social, and intellectual history of the Polish, Czech, and Hungarian states, while he discusses non-state nationalities such as Jews and Ukrainians only briefly, as minorities rather than in their own right.

Wandycz convincingly defines East Central Europe as a "semi-peripheral" zone of Europe, differing substantially from west to east. It has, however, often played an important role in international affairs, economic development, and the arts. Wandycz attributes the beginnings of semi-peripheral status to a late conversion to Christianity that delayed the introduction of Western institutions. East Central European countries gradually overcame their economic, cultural, and political backwardness, but new economic trends pushed it backward again after 1600.

Complex ethnic tensions and the loss of statehood further impeded smooth growth. In modern times, substantial efforts to catch up to the West have met with only partial success.

Wandycz treats the crucial issue of economic development with a sure hand and with attention to historical debate. The region prospered in the fifteenth and sixteenth centuries. Wandycz uses Immanuel Wallerstein's terminology but rejects his view of the region as forming an economic "semi-periphery," dependent on the Western "core," in which the nobles' success in selling agricultural produce and raw materials created an unbalanced economic and political system. Instead, Wandycz endorses contrary arguments by Robert Brenner and Jerzy Topolski that East Central European decline stemmed from falling prices on the Western market in the seventeenth century that ended the agricultural boom and prevented East Central Europe from achieving self-sufficiency in investment capital. Social differentiation sharpened as the nobles, who weathered the storm best, barely recognized the peasants as people and certainly not as members of the body politic. The situation changed little in the eighteenth century, except for Bohemia's growth as the industrial heart of the Habsburg empire, although the Enlightenment challenged earlier assumptions, especially in Poland and Hungary, and paved the way for the emergence of liberalism.

Wandycz finds that the Industrial Revolution of the nineteenth century widened the gap between the West and East Central Europe, despite the region's steady progress. He rejects, again, the interpretation of the region as a semi-colonial dependency, seeing East Central Europe's problem originating in Western neglect, not Western attention. Poverty led to mass emigration and sharpened ethnic antagonisms, particularly against the Jews, who supplied a high percentage of the bourgeoisie. These patterns continued in the period between the two world wars. The problems of adjusting to new boundaries and new markets made it impossible to overturn long-term negative trends, despite much effort and considerable progress.

Wandycz assesses communism harshly for its political and ideological tyranny. He gives a good account of the leading political developments such as the communist takeover, Stalinism, and reform, but he says little about the communist effort to catch up with the West, which made some progress. Communist regimes provided much support for education and culture, even while subjecting the countries to ideological control. Communism also achieved the transition from a predominately rural economy to an urban-industrial economy, even though very imperfectly. As a result, the differences between Western Europe and East Central Europe are probably smaller now than they were before World War II. In addition, communist East Central Europe avoided the commer-

cialism and cheap sensationalism that pervade the West.

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THEODORE H. FRIEDGUT. *Iuzovka and Revolution*. Volume 2, *Politics and Revolution in Russia's Donbass, 1869-1924*. (Studies of the Harriman Institute.) Princeton: Princeton University Press. 1994. Pp. xxi, 514. \$59.50.

This work completes Theodore H. Friedgut's two-volume study of the socioeconomic and political development of the Donbass from the years following the emancipation through the early Soviet period. As such it represents one of the few longitudinal studies we possess of labor and management in a particular industry and region. This important contribution to regional studies is timely, for it lays out the historical context within which the current tensions between Ukrainians and Russians may be understood.

The focus here is on the political forces that shaped Iuzovka in particular and the Donbass in general over the last decades of the imperial period, the nature and outcomes of the political conflicts that erupted in 1917, and the forces that influenced early Soviet political institutions. Beginning with a survey of the structures of political authority operating in the Donbass, Friedgut examines in turn such local representatives of state power as the mining engineers, mining inspectors, and the police, and the principal employers association in the region and its relations with the provincial and district zemstva. He then explores the (modest) development of the labor and revolutionary movements.

From this discussion two basic themes emerge. First, Friedgut shows a society that is fundamentally fragmented and lacking in any real sense of community. Differences in skills, living standards, and religious and ethnic sensibilities separated factory workers and miners, Ukrainian peasants, and Jewish artisans, and undermined the formation of a coherent labor movement, while the near total absence of a worker intelligentsia made it difficult to bridge the gap between the mass of workers and the tiny intelligentsia of the region. Second, Friedgut describes a politically repressive, if often incompetent autocracy that persistently denied the population any experience in civic or political participation. In Friedgut's judgment, the imperial regime proved a poor, inflexible instrument with which to deal with problems of modernization. Indeed, its failure to include the newly awakening forces of society in a reconceived polity emerges as perhaps its fatal flaw, a failure that was to have lasting consequences. But Friedgut also shows the determination of employers to disbar workers from any sort of constructive dialogue about their life and labor: not only did employers consistently reject the legitimacy of any labor organization what-

soever but they also steadfastly resisted Iuzovka gaining municipal status until August 1917.

All this meant that Donbass society was ill prepared for the crises of war, revolution, and civil war. The parochial identities of the working population forestalled the development of a coherent sense of class, while the absence of political experience militated against the creation of a democratic polity. Instead, the Donbass suffered through years of administrative chaos, social disintegration, and extraordinary physical deprivation. Out of this trauma emerged a society built from above by Bolsheviks, not from below by workers, as well as a state that bore strong resemblance to its predecessor: centralized, paternalistic, and reluctant to truly enfranchise its citizens.

As in the first volume, Friedgut gives us enormous amounts of material concerning labor and society in the Donbass. Rarely, however, do we hear the voice of a worker. Indeed, he often portrays workers as recipients of the actions of others, rather than active agents shaping their own environment. When we observe the labor movement, it is usually through the eyes of radical activists or other educated commentators, whose views are often recorded without critical comment. And Friedgut's reliance on the passive voice tends to obscure the issue of agency still more. By the same token, employers, managers, and engineers remain largely faceless. To be sure, he sketches the policies of southern factory and mine owners and articulates the views of leading actors, but the *mentalité* of the people actually running the mines and supervising the shop floor does not emerge clearly.

One leaves this study with a great appreciation for the breadth of material Friedgut has assembled and the contribution this richly descriptive work will make to the study of the crucially important Donbass region. Nonetheless, one wishes he had better sifted through this enormous body of material and woven his analysis more tightly and explicitly into the narrative he spins.

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VLADIMIR N. BROVKIN. *Behind the Front Lines of the Civil War: Political Parties and Social Movements in Russia, 1918-1922*. Princeton: Princeton University Press. 1994. Pp. x, 455. \$55.00.

Two Vladimir N. Brovkins seem to have written this book. One is a serious scholar who extends his earlier work on the Mensheviks to a rich description of the political context in which anti-Bolshevik activism of all sorts played itself out during the civil war. The other is a simple-minded polemicist, tendentious in his misrepresentation of others' work (including my own), analytically shallow, and seemingly ignorant even of the "mainstream" historiography to which he opposes many of his arguments. Throughout, these contending personae interfere with efforts to analyze important material, most of it carefully researched.

The result is a volume in which weak and at times contradictory interpretations are layered on a strong descriptive base, and which can be read most profitably, like the best of the old Soviet historiography, if one ignores the efforts at ideological authority.

The serious Brovkin attempts here "to reconstruct the course of the civil war as a social and political interaction among diverse forces of Russian society," and "to explain the worldview and political behavior of the civil war's key participants" (pp. 8–9). Relying largely on contemporary publications and émigré archives, this Brovkin examines the peasant wars in Ukraine, Tambov, and the lower Volga, the cossack and White milieus, and particularly the tortuous permutations in Menshevik and SR politics, which he presents thoroughly and well. Throughout he concentrates on the unmitigated brutality of Bolshevik repression, none of its horrors new in his retelling, but presented here more compellingly than in most general narratives. Largely through the use of an important if incompletely cited source from the Russian archives (referenced as TsGAOR, f. 130, op. 3, document 414), he attempts as well to reconstruct peasant perceptions and attitudes. He also documents a good deal of labor unrest, largely from published material. This study is not the product of extensive work in former Soviet archives, but it is not weaker as a consequence. Additional documentation would only have embellished its already strong descriptions.

These events are important matters, fundamental to understanding this formative period. Brovkin also discusses the world view of the Whites (without, however, reference to Peter Kenéz), Kronstadt (without regard for the work of Paul Avrich or Israel Getzler), the Bolshevik opposition (ignoring Robert Daniels and Leonard Schapiro), Iulii Martov (without Getzler again), the Cheka (without George Leggett), and the complex problem of worker and, especially, peasant attitudes (with no attention whatever to the extensive historiography on these questions for the prerevolutionary period, and hence to the sociocultural traditions that surely structured civil war outlooks and behaviors). With sufficient space, a reviewer might engage the author's treatment of these issues in terms of, say, the more general problems of state (re)construction, political mobilization and discourse, economic management, or the overarching environment of war and revolution elsewhere in Europe, which, among other things, surely cast both liberal and social democracy in a light different from the one Brovkin uses to illuminate them from his presentist perspective. One wonders, for example, what he means by suggesting that "concerned Communists saw the solution [to the problem of disorder] in restoration of the rule of law" (p. 28), or how he understands the social composition of the party itself in this period, opposed throughout the book to "workers." This is grist for the scholarly mill, the kind of contention that advances understanding.

But what is one to make of arguments about

"revisionists" who have "brought Soviet Marxist categories of social analysis into Western historiography, such as the alliance of workers and peasants, the proletarian dictatorship, and Soviet democracy, as if these notions had existed in reality" (p. 7); or the assertion (p. 8) that "the trajectory of workers' and peasants' movements during the civil war cannot be explained by the Marxist or neo-Marxist social science methodology of revisionists"? Among other consequences of this polemicizing is banality and analytic confusion. We learn that the war between the party and the peasants "had an enormous effect on the future of Soviet Russia" (p. 127); that the closing of *Vsegda vpered* was "to teach [Mensheviks] a lesson" (p. 40); that "the relationship between the ruling party and the workers changed profoundly" (p. 297). There are also passages like the following: "The rebellion of the Tambov peasants . . . is one of the better-known and tragic chapters of the Russian civil war. And yet it remains at odds with the mainstream Western view. For those who are preoccupied with explaining the visionary Bolshevik construction of the future, [it] appears as a reactionary phenomenon: backward peasants resisting the march of Bolshevik modernization" (p. 357). What "mainstream" Western view? The scholarly Brovkin himself allows that "Lenin and Trotsky . . . had been propelled to power two years earlier largely due to their leadership of the workers', soldiers' and peasants' popular movement" (p. 187) and even asserts somewhat dubiously that "the peasants had welcomed the Bolsheviks in the fall of 1917" (p. 241), while the polemicist Brovkin lashes out angrily at "revisionists" who suggest the Bolsheviks may ever have had any social support. From this latter Brovkin we also get quixotic assaults on such "widespread beliefs" as the one holding that Kronstadt sailors "never even tried to coordinate action with workers," minimizing the importance of their rebellion (p. 396). Here is simply muddle, one that the editors at Princeton University Press, if not other readers of the manuscript, should easily have flagged.

At the conclusion of this troubling work, the author decries the tactic whereby all Bolshevik opponents were "automatically labeled with one of the crude Marxist terms invented by the Bolsheviks" (p. 409). One of the crudest of these was, of course, "revisionist," a derisive reduction "invented" by Lenin himself in *What Is to Be Done?* (1902). Like others who have deployed labels in place of serious intellectual engagement, Brovkin compromises his scholarly objectives by failing to heed his own strictures.

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HUGH D. HUDSON, JR. *Blueprints and Blood: The Stalinization of Soviet Architecture, 1917–1937*. Princeton: Princeton University Press. 1994. Pp. xviii, 260. \$35.00.

At least from the time of Jan Wacław Machajski there was an awareness of the prescriptive inclinations of Russian elites, by no means excluding the socialist intelligentsia. Seeking and heeding feedback from the objects of “uplifting” and the readiness to compromise with the various desires and values of the “masses” were not characteristic of the radical intelligentsia or of professionals’ *modi operandi*. That is why the story Hugh D. Hudson, Jr., tells of a small but influential Marxian group of architects who do seek to come to terms with nonelite realities and preferences is so absorbing and, in its own way, encouraging.

Although Hudson’s work is not a definitive history of the entire architectural profession during the first decades of Soviet rule (the 90 percent of architects who were “nonparty” are not discussed), it does spotlight the interactions among those associations of architecture that described themselves as “Marxist” and which held leading roles during the period examined. The Association of New Architects (ASNOVA), which derived its outlook from the ideas of K. S. Malevich, W. Kandinsky, and El Lissitzky, asserted, much in the spirit of the “radical” literary formalists, that particular forms always elicited the same emotional and perceptual reactions. There were “bourgeois” forms and there were “revolutionary” forms and the task of the architect was to identify and utilize “revolutionary” forms to construct the built environment of the socialist society. The very act of seeing these forms, it was implied, would itself imbue revolutionary consciousness. These architects, not surprisingly, were oriented toward the design of monumental public buildings rich in the display of symbols. Interestingly, Anatolii V. Lunacharskii, otherwise a patron of a “soft line” in culture, was a strong supporter of this approach.

ASNOVA’s rival, the Organization of Soviet Architects (OSA), also adhered to modernist canons of style. Its members did not believe, however, in the existence of an eternal aesthetic. Moisei Ginzburg, a leading light of OSA, wrote that the ideal of the beautiful was a social construct, always changing. Moreover, it was more important to change social relations than aesthetic forms, and the architect’s job was to design new structures that would facilitate just those new social relations. Many of their efforts in the early to mid-1920s, consequently, were directed toward workers’ clubs and new kinds of communal housing.

By 1929, after several years of testing, it became clear that workers were not willing to accept new social relations that threatened the coherence of the nuclear family or architecture that promoted such a development. OSA’s leaders therefore began publicly to accommodate their public. They discovered that, in Hudson’s words, “‘the people’ were not a broken machine to be mended by omniscient intellectuals” (p. 59). OSA began to champion a more humane path of industrialization based on single-family hous-

ing and on modest-scale plants dispersed across Russia’s vast territory, as opposed to the Moloch-like conglomerates such as Magnitogorsk.

This defense of the human use of human beings, not surprisingly, was quickly identified by young Stalinists of the All-Union Society of Proletarian Architects (VOPRA) as counterrevolutionary (if we equate the revolution with Joseph Stalin, which they and he did) and by 1935 a major purge of OSA leaders was under way. These young and cruel opportunists had no use for the formalists either, and, taking their cue from the new party czar for architecture, Lazar Kaganovich, enforced a monumentalist style of “baroque” neoclassicism that dominated Soviet construction for forty years.

Despite Hudson’s at times overly Manichean narrative structure (driven by his desire to rehabilitate a humanistic vision of socialism), his book reveals the depth of the struggle between what Arran Gare labeled (*Nihilism, Incorporated: European Civilization and Environmental Destruction* [1993]) the Neoplatonic (Stoic) and more present-oriented or process-oriented (Epicurean) world views not only within the socialist tradition and within cultural modernism but also within all modernizing societies. I have one final caveat. We are all invested in believing that the story we tell is unique. Had Hudson read more widely into the recent literature on the 1920s, however, he would have discovered additional episodes with important similarities in the history of ecology (my work, *Models of Nature* [1988], and that of Gare), engineering (Loren R. Graham’s biography of Petr Palchinskii, *The Ghost of the Executed Engineer* [1993]), and education (Larry Holmes’s *The Kremlin and the Schoolhouse* [1991]). It would also have been interesting had Hudson compared the Soviet experience to that of other socialist architects of the 1920s such as those who built Karl-Marx-Hof in Vienna (described by Helmut Gruber in *Red Vienna* [1991]). Nonetheless, this book is a pleasure to read and will stand as an important account not only in the history of Soviet architecture but also in the history of modernism generally.

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PHILIP G. ROEDER. *Red Sunset: The Failure of Soviet Politics*. Princeton: Princeton University Press. 1993. Pp. xii, 317. Cloth \$49.50, paper \$16.95.

This book is an analysis of the reasons for the collapse of the Soviet Union. The study is history in the sense that it analyzes the development of political institutions from the time of the revolution of 1917 to the present. Philip G. Roeder argues that the emerging contradiction between policy goals and means of implementation, rooted in the initial structure and continuing formation of institutions, resulted in a fatal Soviet incapacity to respond to changing social, economic, and international circumstances.

The study relies on "new institutional" political analysis. This means, essentially, that Roeder focuses exclusively on the role of state and quasi-state institutions such as the Communist Party. Roeder cites theoretical and historical work by the economist Douglass C. North and provides an extensive bibliography of those political scientists who are "new institutionalists" as foundations for the interpretation of historical data. Still, he chooses not to take the obvious additional step of narrating both political and socioeconomic institutionalization.

The book is a fine example of the integration of historical material with the theoretical vision and analytic structure offered by economists and political scientists. An additional strength is the author's willingness to accept the obvious risks of showing how his analytic techniques and findings apply to the political systems of the contemporary former Soviet Union. Strangely, however, the book ultimately relies for its understanding of the nature of the state and the origin of state institutions on such traditional theorists as Samuel P. Huntington. It makes scant use of the more recent and insightful work on the role of the state by political sociologists such as Theda Skocpol, and none at all of major contributors such as Michael Mann.

Unfortunately for its usefulness as history, this book assumes that all relevant events began in 1917. Such a view is myopic, because important aspects of both political institutionalization and bureaucratization can easily be shown to have crossed the revolutionary divide. Roeder's comparatively shallow depth of field forces him into a second problem area: failure to narrate persuasively the origins of political institutions after the revolution of 1917. It is in the course of this narrative of creation of the "Bolshevik constitution" and the emerging Soviet state that he seems to rely most heavily on dated, exclusively political views of the role of the state. A fuller account, which would narrate the historical linkages between pre and post-revolutionary political institutions, and which would make greater use of the large bibliography in Western languages of social and political history of the revolutionary and Soviet eras, might have avoided both of these problems.

Roeder has written a highly useful, if not perfect, political and historical analysis of the Soviet era. In this era of rapidly changing perspectives on authoritarian politics we may eventually hope that historians will be forced to rely on theoretical insights used to such good advantage by Roeder and that political scientists, sociologists, and economists will make still better use of extant historical research.

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RONALD GRIGOR SUNY. *The Revenge of the Past: Nationalism, Revolution, and the Collapse of the Soviet Union*. Foreword by NORMAN M. NAIMARK. Stanford: Stanford University Press. 1993. Pp. xix, 200. \$13.95.

This slim volume focusing on the nationalities question in the Russian empire and the Soviet Union is based on a series of lectures presented at Stanford in February 1991 and then revised for publication following the collapse of the Soviet Union. In the first of four chapters Ronald Grigor Suny stresses the fluidity of the phenomena of "class" and "nationality" in both the Russian imperial and Soviet contexts and notes that until recently the ethnic issue was all but ignored by scholars working in the mainstream of Russian and Soviet studies. In assessing the late-tsarist era and the years of revolution and civil war, Suny argues that the strength of national identity and consciousness among many non-Russian peoples is often exaggerated; in his view considerably more nation-building took place in the Soviet era than in the prerevolutionary years. Echoing a growing consensus among historians of the late-imperial era, he also finds the absence of a tsarist nationality policy, as illustrated by the inconsistency of government actions in dealing with non-Russians.

Suny emphasizes what he sees as an ironic development in the Soviet Union from the early 1920s to the mid-1980s. Despite the imperial nature of the Soviet system and the often negative impact of various state policies on non-Russians, the end result of the "territorialization of ethnicity" (p. 110) was—in many cases—a much stronger sense of nationhood and increasing control of local affairs by native elites.

The final chapter deals with the Mikhail Gorbachev years and his unsuccessful attempt to come to terms with the various non-Russian nationalisms, now increasingly empowered by the policies he undertook. Suny is sympathetic to Gorbachev and feels that historical evaluations in the future will probably be kinder to him than present assessments. The last Soviet leader's "revolution from above" was "hijacked by nationalist revolutions from below" (p. 132), as even a Gorbachev-led transformation into a genuine federation could no longer satisfy a number of union republics that had opted for full independence: the Baltic states, Moldavia, Georgia, and Armenia.

Viewed as a series of lectures, the material presented here is thought provoking and wide ranging; it is, however, considerably less successful as a book. For example, Suny's attempt to classify nine major nationalities in the late Russian empire according to their identification with class or nationality remains unconvincing. The categories are too stark, lacking in nuances, and they are presented ahistorically, that is, without noting the possibility of change over time in the decades before 1917. Thus, it is not accurate to refer to the "almost completely peasant composition" of the Lithuanians; nor did they have a "low level of national consciousness" (p. 30) by, say, the revolution of 1905. Regarding the Latvians, it is misleading to argue that the Social Democrats were the totally dominant political force both before and after 1917.

The potential pitfalls in generalizing about the Soviet nationalities question, given all its complexity,

are also evident in such a brief survey. The positive benefits of Soviet-style modernization that the author alludes to, for example, "nativization" in culture and political leadership or economic and social transformation, were not evenly distributed among the major nationalities but rather varied significantly by region or even by individual republic. Furthermore, this view does not take into account the experience of the non-union republic nationalities (such as Tatars, Jews, and Germans) who lacked the institutional bases under Soviet rule for consolidation and development of their national identity.

Overall, however, there is much of interest here, especially Suny's exploration of the interconnection of ethnic and social issues in modern Russian history. Above all, this book should contribute to placing the long-neglected nationalities question in the mainstream of historical studies on the Russian empire and the Soviet Union.

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NEAR EAST

GUILAIN DENOEU. *Urban Unrest in the Middle East: A Comparative Study of Informal Networks in Egypt, Iran, and Lebanon*. (SUNY Series in the Social and Economic History of the Middle East.) Albany: State University of New York Press. 1993. Pp. x, 310. \$17.95.

In this elegantly argued study, Guilain Denoeux makes an original and persuasive contribution to the debate over the political consequences of urbanization in what used to be called the Third World. Post-World War II modernization theory proposed that "floating masses," disconnected from their traditional cultural roots, would generate tension and turmoil. In the 1970s this proposition was challenged by a new generation of social scientists who emphasized the "ruralization of the city" and the persistence of a stable cultural order through the agency of informal networks. Denoeux takes the debate to a new level by arguing persuasively that informal networks, while they can serve some stabilizing functions, also can serve as vehicles of political protest, especially in repressive political systems. He accepts the conventional modernization notion that urbanization leads to social dislocation and psychological stress, and he admits part of the revisionist argument that informal groups are formed in the expanded urban setting in order to reintegrate dislocated individuals and marginalized populations. But he goes on to argue that they do not always succeed. Why? Because they "are sometimes tied to counterelites that have access to resources that are not controlled by the authorities, and . . . are based on alternative visions of society, and therefore present a direct or potential challenge to the existing political order" (p. 24).

Although Denoeux's thesis is theoretically applica-

ble to the Third World, it is grounded in the Middle East. Even within the Middle East, of course, there are significant social and historical variations, and it is a challenge for the comparativist to generalize about it without taking into account historical specificities. Denoeux meets this challenge in the second part of his study, which begins with a concise account of the unincorporated social structure of the traditional Islamic city and then discusses the historical evidence for networks as a vehicle for political protest, principally in Iran, Egypt, and Lebanon, as well as from Turkey and Ottoman Syria and Iraq. Drawing on the literature, Denoeux nicely compares and contrasts various clientelist and neighborhood groupings such as Iranian *lutis*, Egyptian *futuwwat*, and Lebanese *qabadayat*. He argues that the Iranian state was largely unable to dominate informal groups, while the Egyptian state under Muhammad Ali and after was much more successful in doing so, an important point for political scientists to consider as they study the contemporary challenge from the *jama'at islamiyya* to President Hosni Mubarak's regime. In Lebanon, by contrast, the state has been exceptionally weak and has sought to build on, rather than manage or suppress, clientelist groupings, a strategy that contributed to the country's recent fifteen-year civil war.

It is in this part of the book, which deals with evidence since the 1940s, that Denoeux drives home his argument. First, he discusses the influence of formal opposition groups and suggests that it has generally been eclipsed by that of informal groups because the increasingly powerful state can more easily identify and suppress the former type. He then looks at the destabilizing effects of informal patron-client groups in a weak state—Lebanon—and in Iran as the shah's regime began to lose legitimacy. A third chapter is devoted to the informal networks in the Iranian bazaar, explaining both the cohesion of the bazaaris and the inability of the shah's security organization, SAVAK, to crush their resistance. He concludes with an insightful comparative analysis of religious groupings in the three countries. Along the way he posits the notion of "double-edged" groupings that, under certain circumstances, switch from a supportive to a subversive function.

This study does not present new field research. Thus, it is captive, in a sense, to the existing empirical literature and accepted interpretations. Specialists will have their particular reservations. Rather, its value lies in its elegant and concise reinterpretation of existing literature on informal groupings. Without lapsing into banal or excessive simplification, Denoeux is nonetheless able to schematize the essential characteristics of different networks (see, for example, pp. 59, 100, 156, 167, 174, 212, and 213). And without indulging in vulgar essentialism he succeeds in specifying the conditions (such as Islam, urbanization, and the strength or weakness of the state) that can turn informal networks from system-supporting to system-subverting structures. Political scientists as

well as historians will find this study a significant contribution to our understanding of stability and legitimacy in Middle East politics.

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MORDECHAI BAR-ON. *The Gates of Gaza: Israel's Road to Suez and Back, 1955–1957*. New York: St. Martin's. 1994. Pp. xi, 404. \$35.00.

This study of Israeli diplomacy during the era of the Suez crisis, originally published in Hebrew in 1992, is a perceptive and thorough monograph that addresses interesting questions, presents new evidence, and reaches compelling conclusions. Perhaps no one is better qualified than Mordechai Bar-On to write such a study. A high-ranking officer in the Israeli Defense Forces (IDF) in the 1950s who earned a doctorate in international relations thirty years later, Bar-On examines multinational primary records with the informed eye of one who personally remembers the moods, arguments, and personalities that shaped policy but remain shrouded in the archives.

This book focuses on Israeli security concerns from the time of the Egyptian-Soviet arms deal to Israel's evacuation of Egyptian territory after the Suez War. Bar-On explains that Prime Minister David Ben-Gurion, fearful of Egyptian aggression after the Soviet arms deal, approved a reprisal raid against Syria in December 1955 but blocked the IDF's plans to provoke a preventive war against Egypt because "Ben-Gurion did not want to test the Americans" (p. 51). Bar-On surveys Israel's cautious reactions to foreign peacemaking efforts, such as the Anglo-American Alpha plan, the peace missions of Robert Anderson, Eric Johnston, and Elmore Jackson, and the arbitration efforts of United Nations Secretary General Dag Hammarskjöld. Although he makes a case that these Western peace initiatives were hobbled by erroneous assumptions, scholars who find some merit in them will question his dismissal of the Alpha initiative as "pathetic," "naïve," and "more than slightly ridiculous" (p. 89).

With insight and sensitivity, Bar-On examines the policy dilemmas that confronted Israel in late 1956 and the reasons it decided, with France and Britain, to attack Egypt. In his view, the U.S. indirectly encouraged Israeli involvement. President Dwight D. Eisenhower's denial of arms to Israel led the country into a partnership with France that provided weapons but also involved it in collusion. International criticism of reprisals against Jordan convinced IDF planners "to go for broke" (p. 218) against Egypt.

Bar-On's examination of the American-Israeli showdown over the terms of Israel's withdrawal from occupied Egyptian territory is another strong feature of his book. He concludes that Eisenhower's televised address demanding unconditional evacuation profoundly influenced Israeli officials, who interpreted it as a "declaration of war" (p. 297). Initially defiant,

Ben-Gurion realized that he had to concede and skillfully negotiated the best possible terms. Because Egypt gained political prestige and Israel won ten years of peace, Bar-On observes, "both Egypt and Israel emerged [from the war] as victors, even though they fought against each other" (p. 323).

Bar-On demonstrates strict, perhaps excessive, detachment on controversial issues. He criticizes neither Ben-Gurion nor Hammarskjöld in his account of an Israeli-U.N. dispute over IDF reprisals, admiring both men for defending their vested interests. He deems Israeli fears of Egyptian aggressiveness excessive but acknowledges that to contemporary officials the threats seemed real. He exonerates Ben-Gurion for angering the United States during the Suez War on the grounds that no Israeli could have foreseen U.S. reaction to the hostilities. Some readers might wonder, however, whether Israeli officials should have better monitored Eisenhower's numerous warnings against force, as well as his temperament and insulation from political pressure groups. What other reaction might Ben-Gurion have expected from the administration that had devised the Alpha plan and denied Israel weapons?

These questions aside, Bar-On makes a substantial contribution to the recent wave of books on the Suez crisis by examining the Israeli dimension on an unprecedented scale. His work is essential reading for scholars studying Israeli policy in the critical middle 1950s.

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ASIA

JAMES CAHILL. *The Painter's Practice: How Artists Lived and Worked in Traditional China*. (Bampton Lectures in America, number 29.) New York: Columbia University Press. 1994. Pp. xi, 187; 117 plates. \$32.50.

In this volume, which expands the Bampton Lectures he delivered at Columbia University in 1991, the distinguished art historian James Cahill examines the social context within which Chinese painting evolved. Cahill selects four aspects of the subject for intensive scrutiny. He begins with a superb overview of the historiography, arguing that the Chinese literati view of painting as a mode of self-cultivation and object of aesthetic contemplation emerged in the late eleventh and early twelfth centuries and completely dominated the field by the fourteenth century. Literati criteria shaped the development of Chinese painting, dictating preferred subjects, brushwork, and the higher valuation of amateur over professional painters. Paintings were decontextualized, their original meaning and purpose ignored; the major criteria of value became age and authenticity. Discussions of artists' fees, prices of paintings, and the notion of a market for art were censored from writings about painting. Because the same criteria were transmitted to (and

accepted by) painting specialists in the twentieth century, Western scholarship until recently has focused almost exclusively on the paintings of "amateur" literati rather than professional painters, and on painting techniques to the exclusion of socioeconomic analysis. Moreover, as Warren Cohen has shown (*East Asian Art and American Culture: A Study in International Relations* [1992]), the same judgments profoundly influenced the development of Asian art collections in the United States.

Subsequent chapters focus on long-neglected aspects of painting production. "The Painter's Livelihood" first cites the occasions on which paintings would have been presented and displayed, then describes the direct and indirect modes of commissioning or buying a painting. The elaborate ways in which payment could be disguised testify to the dominance of the literati values noted above. "The Painter's Studio" investigates the relationship between artists and clients, which was determined by their relative social statuses. Analyzing how paintings were produced and who (in addition to the named artist) helped produce them, Cahill points out that even the most prestigious artists produced multiple copies of a work; many works commemorating birthdays, weddings, retirements, and leave-takings were painted within a conventional format; and even good painters produced work of inferior quality to meet their need for small gifts to present on social occasions. "The Painter's Hand" looks at the consequences of the literati painting conventions for Chinese painting, as technical skill and the ability to accurately render objects in real life gave way to the amateur painting ideal of "sensitive and unassertive" brushstrokes in a refined and controlled rendition of a narrow range of subjects. Yet the amateur ideal did not insist that an artist actually paint all of the works that were attributed to him. Amateur and professional painters hired "ghostpainters" to help fill the incessant demand, and collectors tolerated this practice because the finished work was still assumed to bear "traces of the painter's hand."

By breaking with the accepted truisms that have dominated the field of Chinese painting, Cahill's work raises stimulating questions for future research into the relationship between market forces and artistic production. Like Craig Clunas, who argued for the birth of a consumer society in sixteenth-century China (*Superfluous Things: Material Culture and Social Status in Early Modern China* [1991]), Cahill points to the commodification of art as an outcome of the commercial boom in China during this period. His book is an excellent example of the ways in which the incorporation of socioeconomic analysis can bring fresh insights to the examination of Chinese painting.

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TIMOTHY BROOK. *Praying for Power: Buddhism and the Formation of Gentry Society in Late-Ming China*. (Harvard-Yenching Institute Monograph Series, number 38.) Cambridge, Mass.: Council on East Asian Studies, Harvard University, and the Harvard-Yenching Institute; distributed by Harvard University Press. 1993. Pp. xvii, 403. \$35.00.

With this monograph Timothy Brook makes a substantial contribution to our understanding of a lamentably neglected subject area: the place of Buddhism in late-imperial Chinese culture and society. Taking as his main concern a distinct rise, perdurance, and decline in patronage of Buddhist monasteries by local elites (the so-called "gentry") in China from the mid-sixteenth through the end of the seventeenth century, he explores a wide range of political, cultural, social, and economic factors in this phenomenon, while also painting richly colored landscapes and portraits of the time.

Brook begins with a helpful introduction to "gentry studies," explaining the pertinence of monastic patronage to certain issues therein. In part 1, "The Culture of Buddhism," he ably reviews developments in Ming-period religious philosophy (as well as in politics) that resulted in increased openness to and familiarity with Buddhism among local cultural arbiters, whose strongest commitments usually were to Confucianism. In part 2, "Monastic Patronage," he analyzes the principal forms of gentry contributions: land donations; money and materials for building and renovation; exercise of social and political influence to forward and protect monastic interests; and "literary patronage," the composition of admiring poems and essays or the compilation and printing of an institutional history to elevate the prestige of a given monastery. In this section he also closely examines the bases of appeals for patronage, mentioning gender, kinship, and religious ideas, but finding most interest in social and cultural appeals. Part 3, "Patronage in Context," offers detailed studies of the conditions of gentry life and monastic patronage in three different counties: a poor one where Buddhist institutions were not well developed; a rich one in which monasteries flourished to the greatest extent apart from the national capitals; and one in peculiar circumstances that allow Brook to highlight the ambiguous position of the country magistrate vis-à-vis monastic patronage. The nonspecialist will find this last part heavy going, but it will be appreciated by those who specialize either in the period or in local and regional studies.

The core source material of this book is a large body of local and monastic histories, usually called "gazetteers" (*difang zhi* and *sizhi*), and Brook masterfully shows what can be accomplished through assiduous mining of this genre. Also commendable is Brook's pursuit of his subject through and beyond the disruptions of the Ming-Qing dynastic transition, the confusions of which most researchers prefer to avoid.

His style of writing, too, is brisk and lucid, sustaining the reader through even difficult sections of this thick monograph.

Problematic are Brook's disinclination to accord full status as a historical factor to religious philosophy or belief, and a lack of clarity in what is meant by "the formation of the late-Ming gentry." At the outset Brook states, "This book is about neither religion nor its institutions" (p. 2). Yet he follows with a long section on interactions between Ming neo-Confucianism and Buddhism, only to deny at several points that belief was a primary motivating factor in the groundswell of monastic patronage that occurred in exactly the same timespan as the thought trends described. His insistence, made clear in the introduction and elsewhere (for instance, pp. 16, 88, 90, 317) that changes in religious philosophy were secondary or posterior to, derived from, or simply provided a convenient hermeneutical adjunct to the changing social, political, and economic circumstances of the local elite, can be challenged with material that Brook himself presents. More appropriate would have been a cybernetic approach to relations between thought and society.

Key to Brook's conception of a certain gentry formation in late Ming and early Qing is "autonomy." He argues that the gentry, with increased landed and commercial wealth but decreased chances of holding bureaucratic positions, were eager to enlarge any neutral space between themselves and the state, and that patronage of Buddhist monasteries provided a culturally sanctioned means of doing that, as well as a means of distinguishing the true local elite—moneyed, refined, and community-spirited—from the burgeoning parvenu elite of those who merely held the lowest civil-service degrees. Brook's assertion that monastic patronage not only proved to be handy "cultural packaging" for the aspiring true elite but that it also laid groundwork for the more extensive, organized gentry maintenance of nonreligious local facilities and services that characterized the eighteenth and nineteenth centuries is stimulating. He overstates, however, the degree of autonomy from the state that Buddhist institutions could provide in Ming or Qing times, as well as the degree of autonomy that local elites desired. Overtly political activities in monasteries, for instance, never would have been tolerated; and the prime criterion for distinguishing who was more elite than whom remained the possession of higher civil-service degrees that actually qualified one to serve in the bureaucracy. That monastic patronage was dropped with alacrity as soon as the Qing state achieved good working order suggests that Brook's "formation" was just a temporary response to politically disrupted times.

Deeper, more sustained comparison of the late-Ming gentry with that stratum in Song and middle-Qing times, as well as with other periods of weak government, could have helped clarify what Brook thinks was distinctive about the late-Ming "forma-

tion." And at least some attention to the Tang period would have precluded the statement, "Prior to the Ming . . . monastic patronage had little to do with power" (p. 33). Nevertheless, this is an important, well-executed study that will profitably be read by those interested in the social history of religion, the cultural history of philanthropy, and the general history of late-imperial China.

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STEWART GORDON. *The New Cambridge History of India*. Volume 2, part 4, *The Marathas 1600–1818*. New York: Cambridge University Press. 1993. Pp. xv, 202. \$44.95.

The Marathas occupy a problematic place in the history of India. Colonial historians, notably Grant Duff, while acknowledging them as Britain's immediate predecessors, characterized Maratha conquests and rule as turbulent and predatory. The Maratha polity was portrayed as emblematic of a dark time of troubles from which British rule had rescued India. Maharashtra's own historians vigorously contested this interpretation. For example, negative portraits of the Maratha's "founder," Shivaji (1630–80) were countered by his representation as an Indian nationalist, a champion of Hinduism against the "foreign" Mughal empire and other Muslim-led sultanates, or, later, a reformer dedicated to social justice for the non-Brahman majority population. Maratha history continues as a vehicle of social, political, even moral discourse down to the present day in Maharashtra.

Stewart Gordon, one of a handful of non-Indians to make recognized contributions to the study of Maratha administration and rule, here presents an intelligent and insightful new synthesis on the Marathas and their polity. Opening with an illuminating review of the geopolitics of the medieval Deccan, he emphasizes continuities of social and political conditions and problems from earlier Muslim-ruled states through the rise of Shivaji's kingdom to the emergence of the eighteenth-century Maratha dominion over much of the subcontinent. If in the process, Shivaji's mythic dimensions are diminished, Gordon's quiet, reasoned exposition permits a more nuanced appreciation of what the Maratha king attempted and accomplished. In an analysis of the role of Maratha warrior families (*deshmukhs*, local magnates sharing "nested rights" over territories nominally subject to a more distant sovereign), Gordon underscores the dynamic tensions that operated across the time and space of Maratha rule. Loyalty and legitimacy were fluid and contested. The early Maratha kingdom was colored more by contingency than by protonationalism. Modern communalist images of "Hindu" Marathas battling "Muslim" foreigners do not bear scrutiny when examining the composition of the sides of the many Deccan conflicts.

Post-Shivaji intra-Maratha rivalries permitted the

Mughal emperor Aurangzeb and his army to invade Maharashtra. Yet, conversely, they also prevented a definitive Mughal annexation. Less than a decade after Aurangzeb's death in 1707 (not at Aurangabad [p. 103], but Ahmadnagar) Maratha power re-emerged, led by a dynasty of able Brahmans holding the hereditary post of *peshwa* (chief minister). Building from his earlier research on Maratha administration of conquered territories in central India, Gordon emphasizes that although Maratha expansion began with raiding and looting, settled revenue and judicial administration soon were established. He also explicates a gradual transformation of the Maratha military as it adapted Mughal heavy cavalry and field artillery tactics, a process that made compelling demands on the size and regularity of revenue collection. The costs of military change as well as the challenge of European and Asian foreign powers ultimately brought the Maratha dominance to an end. After a brief chapter outlining the British conquests, Gordon concludes with a thoughtful review of themes and cycles in Maratha history and institutions. Without explicitly suggesting how the Maratha polity fits into a typology of state formations, he does highlight Maratha's revenue and information gathering as a foreshadowing of British colonial practice.

Gordon's fresh synthesis has been possible in part because of recent availability of Maratha state and family papers. It is therefore to be regretted that he was compelled, presumably by imposed limitations on length, to not provide much analysis on society and culture in Maharashtra under the Marathas, and to not convey a sense of everyday life. What were the consequences of Maratha rule for villages? He mentions relative openness of caste and religious innovation, but detailed examples would have been welcome. Nonetheless, this book offers a brilliant and compelling reading of the Maratha's political and military enterprise.

Perhaps like Maratha rule itself, this excellent book has its lapses. Although they do not vitiate the interpretive contribution, there are odd mistakes (for example, Shivaji's traditionally accepted birth date in 1627 is attributed to the "Jedhe Chronology," although it was that text that first identified 1630 [p. 59]). Misprints produce errors and inconsistencies of spelling and dating in the text, footnotes, and index; a chapter title is garbled (p. 91) and at one point (p. 60) Shivaji is transposed with his father Shahji. Whatever became of copy-editing?

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ANITAINDER SINGH. *The Limits of British Influence: South Asia and the Anglo-American Relationship, 1947–56*. New York: St. Martin's or Pinter, London. 1993. Pp. xvi, 309. \$49.95.

In the historiography of the Cold War, South Asia is an often neglected area. Anita Inder Singh partially

fills this gap in this book. She claims that Britain's withdrawal from the Indian subcontinent was more spontaneous than historians usually suppose. Britain granted India independence because it could neither afford to hold the subcontinent with its own troops nor rely on Indian officials in the face of the Indian National Congress's overwhelming popularity. This reason for the rapid end of the Raj explains why, immediately after independence, Britain had to try harder than it had expected to preserve its diplomatic influence over India and Pakistan. But by 1949 Britain's rulers knew well that the Commonwealth would never amount to a "Third Force" equal to that of the United States or the Soviet Union.

Nevertheless, India and Britain had much in common. Both of them opposed communism, but neither of them viewed the world in the strictly bipolar terms that the United States did. Whereas the United States saw containment of conflict as a means to the end of defeating communism, Britain and India saw it as an end in itself. Furthermore, historical experience led Britain to share India's view of South Asia as the strategic center of Asia, but it led the United States to regard the Pacific Rim as the focus of activity in the Far East. Underlying these similarities in strategic perspective between Britain and India was Britain's lack of material and human resources, which forced it to adopt a cautious diplomatic approach.

The result was that in the early 1950s both Britain and India pursued more conciliatory policies toward communism in Asia than did the United States. Britain had learned from its experience with India's independence movement and its relationship with the Indian government that had sprung from that movement. These lessons made Britain more tolerant of nationalism in Asia than the United States, even when nationalism arose wrapped in communist ideology. Both India and Britain, therefore, recognized Communist China and negotiated for the establishment of independent neutral states to succeed French Indochina.

By 1951 the United States regarded India as a "baneful" influence on Britain that was eroding the Anglo-American relationship. Having long since abandoned hope of recruiting India into an Asian anticommunist alliance, the United States focused instead on drawing Pakistan into a military pact centered in the Middle East. This approach suited Pakistan, which had already fought a war against India in the late 1940s and continued to fear India's demographic and military superiority. American military aid to Pakistan signaled the end of British influence over the Muslim dominion. In 1956 Britain antagonized both India and the United States by participating in the invasion of the Suez Canal zone. The Suez crisis marked the end of Britain's attempts to assert world-wide influence on the basis of its former imperial geography, which had centered on India.

This topic is not a new one for Singh. Since 1982

she has published several article-length studies of the strategic implications of South Asian independence. She has incorporated much of the information and many of the arguments from these previous publications into her book. But by looking at the entire sweep of the relationship between Britain, India, Pakistan, and the United States from South Asia's independence to the Suez crisis, she offers a broad perspective that her earlier works could not.

Singh's approach to diplomatic history is a traditional one. She provides ample citations to justify her arguments, and she stays close to her sources. In doing so she adds to the growing body of diplomatic histories that challenge the bipolarity of the Cold War and the solidarity of Britain's "special relationship" with the United States. Her work is a valuable addition to the historiography of the early Cold War.

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GERALD CANNON HICKEY. *Shattered World: Adaptation and Survival among Vietnam's Highland Peoples during the Vietnam War*. Philadelphia: University of Pennsylvania Press. 1993. Pp. xxxiv, 297. Cloth \$34.95, paper \$14.95.

Scholars of Vietnam of the English-speaking world owe a great deal to Gerald Cannon Hickey; he was the first American anthropologist to publish a full-length ethnographic study of a Vietnamese village (*Village in Vietnam* [1964]). As Hickey's interest in what was South Vietnam lingers, it is inevitable that his attention should have been directed to the ethnic minorities that live in the mountains along the central coastal plain of Vietnam. That interest was shared, in the same period, by the CIA, the U.S. Marines, and the Summer Institute of Linguistics, each organization pursuing, of course, its own ends. Blending the findings of their research with his own, which he has accumulated during many years of fieldwork in the region, Hickey has already published two fine works: *Sons of the Mountains: Ethnohistory of the Vietnamese Central Highlands to 1954* (1982), and *Free in the Forest: Ethnohistory of the Vietnamese Central Highlands, 1954–1976* (1982).

In the book presented here, Hickey describes ten of these highland peoples. He does that by giving, for each of these groups, a straight ethnographic description encompassing their religion, settlements and houses, family, kinship system, economic activities, village leadership, and the effects of the Vietnam War.

Although I can easily recognize the quality of Hickey's scholarship as shown in the richness and accuracy of the data contained in this book—he is undoubtedly an authority on the ethnic minorities of South Vietnam—I fail to see the usefulness of this publication, which brings no real new information compared to the content of the two volumes mentioned earlier. Can the publication of this book be justified by a more systematic, logical, and clearer

presentation of the data, which consists of giving all the information he has accumulated concerning each ethnic group in a special chapter? Possibly, although I detect some inexplicable flaws in the organization of these chapters. For some reason, Hickey devotes only eight chapters to the ten groups. The Roglai are discussed in the same chapter with the Chru and the Bru with the Pacoh. Within these chapters, surprisingly enough, the author writes separately, one after the other, about the Roglai and the Chru, and about the Bru and the Pacoh. Another anomaly is the fact that the author concludes five of his chapters with a page or two on the impact of the Vietnam War on the particular ethnic group studied in that chapter. But for the Sedang, the Jeh, and the Halang (the subjects of chapters 6, 7, and 8 respectively), the author, for no apparent reason, reserves all of chapter 9 to discuss the consequences of the war on these three groups.

Finally, I believe that the subtitle of the book is inappropriate. Of almost three hundred pages of a book subtitled "Adaptation and Survival among Vietnam's Highland Peoples during the Vietnam War," the author devotes fewer than forty-five pages to the impact of the war. In fact, the subtitle should also have pointed out that the book deals only with the highland peoples of South Vietnam and not of the whole of Vietnam. As for the role played by the Vietnamese vis-à-vis the highland peoples, is there nothing other than killings during the war and oppression after the war? Here I think Hickey lacks first-hand information concerning the policies practiced by the Viet Cong during the war and by the Vietnamese government after the war.

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UNITED STATES

CHARLES J. FARMER. *In the Absence of Towns: Settlement and Country Trade in Southside Virginia, 1730–1800*. (Geographical Perspectives on the Human Past.) Lanham, Md.: Rowman and Littlefield. 1993. Pp. xiii, 208. Cloth \$57.50, paper \$25.00.

"Country stores . . . were critical parts of evolving settlement systems and should be approached in terms of geographical theory" (p. xi). Although he recognizes the importance of towns and their economic, political, and cultural influences beyond their bounds, Charles J. Farmer points out that in 1800 no more than 10 percent of the American population lived in "urban" areas. The South had relatively few towns; thus, the country store provided crucial services to the residents of Southside, or Piedmont, Virginia. This region preserved the frontier-mercantile system characteristic of pioneer economies into the nineteenth century.

The author first presents various concepts and

theories employed by geographers, which, for historians, is useful in placing this study in context. The theoretical framework is based on three general concepts: "central-place theory, the mercantile theory of trade and settlement, and the staple theory of settlement and economic development" (p. 5). Farmer then analyzes the development of the region, tracing migration through such data as land sales, road construction, and the establishment of churches, grist mills, and ordinaries. Excellent maps clearly illustrate settlement patterns. Because the population remained dispersed and decentralized—the growth of slavery also contributed to the lack of a population density sufficient to support urbanization—plantations, ferries (often tobacco inspection sites), and county court meetings met the trading needs of the region.

Farmer next examines the economic activities pursued by Southsiders and explains why the concentration on the production of staples that did not require local processing contributed to the absence of towns. Marketing, and especially the dominance of Scots in the tobacco trade, and transportation systems are also analyzed.

Building on these data, Farmer, in the most important contributions of this work, divides mercantile patterns into four periods: the formative period, Scottish dominance, the revolutionary period, and the postwar years. Mercantile activities and consumer choices receive detailed analysis; Farmer stresses the various options available to businessmen and residents at different places and times. Itinerant wagon merchants and peddlers were perhaps as important as fixed places of conducting trade. Country store locations depended in part on the quality of transportation available in this landlocked region. Retailing activities also depended on seasonal production, initially of tobacco, and then of wheat. Not surprisingly, the more affluent people had access to goods that were imported from urban areas or from abroad; they also had greater access to credit. Fabric, food and drink, hardware, and household goods were major purchases. Slaves were rarely sold at country stores.

In his final chapter Farmer returns to theory. He convincingly rejects the "central-place" theory of economic development and trading patterns for Southside, Virginia. Rather, the country store continued to be the focal point for a complex network of business relationships carried out over relatively long distances.

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FRANK LAMBERT. *"Pedlar in Divinity": George Whitefield and the Transatlantic Revivals, 1737–1770*. Princeton: Princeton University Press. 1994. Pp. xii, 238. \$24.95.

For generations professional historians dismissed George Whitefield as, in Perry Miller's characteristic terms, the "pied piper" of revivalism. Unlike

Jonathan Edwards, who commanded intellectual admiration, or John Wesley, who founded America's largest Protestant denomination, Whitefield appeared on the sidelines as a highly egocentric, shallow-minded demagogue. In the past decade, however, all that has changed as scholars have begun to explore eighteenth-century "popular culture" and the dynamics of a market economy. In these terms Whitefield reemerges as a central character, less a pied piper than the Benjamin Franklin of modern, evangelical media. More than any of his peers in religious organizations, Whitefield showed how to make evangelicalism popular, capable of reaching ever-expanding audiences of anonymous strangers, most of whom he had never met. Modern revivalists from Charles G. Finney to Billy Graham all profited from new measures first introduced by Whitefield. In this book Frank Lambert pulls together many of these recent themes and adds entirely new insights that are certain to fan the fires of Whitefield's recovery.

Central to Lambert's analysis is the concept of an eighteenth-century "commercial revolution" that created new mass markets in everything from ceramic pottery to newspapers and magazines. In religious terms, this revolution produced a climate in which innovative messages might be heard and, more importantly, read in mass, impersonal settings. Where earlier scholars posited an irreconcilable chasm between print and aural culture in the eighteenth century, Lambert shows how, in the case of Whitefield, print reinforced his spellbinding oratory so perfectly that he was able to create what was, in effect, a religious market centered around himself as the chief promoter and distributor.

From his extensive research in British archives and newspapers on both sides of the Atlantic, Lambert provides singular insights into Whitefield's religious and journalistic innovations that are far ranging in their applicability. By tracing closely the process by which Whitefield borrowed merchandising techniques to achieve religious ends, Lambert shows how notions of the religious "public" achieved new meanings. As such, his work represents an important supplement to the work of social theorists like Jürgen Habermas and Alasdair MacIntyre.

Lambert occasionally runs the risk of overstatement. If market revolutions and market terminology were important to Whitefield's organization and even to his sermonic language, as they most surely were, they were not as all-encompassing as Lambert's model would imply. A random count of market terms in Whitefield's printed sermons reveals occasional economic and market terminology appealing to "bankrupt" sinners and pointing to a savior who "paid the debt," but not in any single-minded or preponderant frequencies. If conversions were evangelical Protestantism's equivalent to sales in the marketplace, that is not the way evangelicals thought of them. Neither is it what Whitefield thought, particularly as he became older and more reflective.

The older and more reflective Whitefield does not appear much in these pages. Although Lambert is the first to insist that his work is not a biography in the classic sense of the term, readers are still apt to read it as one, leading to some possible misimpressions. Too often the young Whitefield is allowed to stand for Whitefield generally, and too often this narrowing of focus leads to unfortunate repetitions that a good editor should have identified. Twice (on consecutive pages), we learn that Whitefield "developed [an] intercolonial evangelical network" (pp. 108, 109). And twice we read the same story of Franklin's famous estimate of Whitefield's audience size (pp. 63, 113).

The weaknesses of this monograph—more of coverage and emphasis than interpretation—are shared with other recent accounts and analyses. But the strengths are singular and impressive. More than any other work, Lambert's provides the foundation for a new understanding and appreciation of the life and times of Anglo-America's greatest evangelical revivalist.

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JACK FRUCHTMAN, JR. *Thomas Paine and the Religion of Nature*. Baltimore: Johns Hopkins University Press. 1993. Pp. xiii, 215. \$32.50.

Jack Fruchtman, Jr.'s study is not just another biography of the familiar Tom Paine; although grounded on earlier studies—especially Eric Foner's *Tom Paine and Revolutionary America* (1976)—it is focused on Paine's "religion of nature," a view of the world that started with reverence for nature itself, encompassed the contradictory tenets of deism and pantheism, and stretched to late Enlightenment human faith in invention, virtue, and perfectibility. Paine, says Fruchtman, was a "wandering homiletic writer" (p. 15) whose theory of natural rights led to attacks against "false systems and false principles" (p. ix), and directed American and French revolutionists alike toward a "enterprise of freedom and democracy" (p. xi).

Religious themes and imagery, filtered through pastoral, political economic, and millennial languages, were the leitmotif of Paine's writings over the years. Much of classical republicanism, however, is not very useful for understanding Paine, argues Fruchtman; Paine directed republican tenets of representative government and virtue to middling and lower social classes, and reserved his harshest invective for the aristocratic Burkean prescriptive traditions that had affinity with republicanism's elitist components. Paine's incorporation of Smithian and Humean admiration for unfettered commerce—held in check by individual moral sense—looked forward to nineteenth-century liberalism. He was a prophet of progress who rejected classical republicanism's static view of the economy; in the context of a return to God-given natural rights and use of a very Lockean

universal ability to improve, Americans and French would lead the way to rejecting unnatural tyranny, recovering the civil rights of citizens in society (including those of unequal private property and male-only suffrage), and remaking governments that would undertake both to redistribute resources from wealthy to poor and to promote invention, creation, expansion.

Fruchtman offers feasible explanations for some of the supposed contradictions that previous scholars puzzled over in Paine's writings. For example, Paine could simultaneously criticize the British national debt because it was grounded on both war and aristocratic privilege, and yet support the Bank of North America because an American national debt would be founded on the vast wealth of a national domain that became more valuable with continued settlement and improvement, thereby creating bonds of national unity as well. The Lockean labor theory of value thus resolves what is only an apparent contradiction in Paine. In another example, Fruchtman explains how Paine could oppose paper money—a favorite demand of farmers during the era—and also offer unflagging support for the poor; the apparent contradiction dissolves when one understands that Paine believed all unnatural inequalities would disappear (although any natural ones would remain), and prices would stabilize, if exchange became grounded on its most natural form, gold and silver. In a third example Fruchtman settles the issue of Paine's early association with nationalists and his disaffection with Federalists by 1802. Those who had at first offered Americans the promise of rising prosperity—a vision of modernity consistent with Paine's greatest hopes for humanity—failed to embed this vision in civil society's institutions and halls of political power. When Federalists suppressed political dissent—a right as dear to citizens as natural rights were to all individuals—they became as unnatural as the aristocracies of Europe.

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MICHAEL A. BELLESILES. *Revolutionary Outlaws: Ethan Allen and the Struggle for Independence on the Early American Frontier*. Charlottesville: University Press of Virginia. 1993. Pp. xi, 428.

Michael A. Bellesiles's Ethan Allen is an exemplar of republican values and a deist who could flourish in a Vermont so religiously diverse and contentious that it became a haven for nonconformists. With a magnetic personality and a talent for self-promotion, his renown as the captor of Fort Ticonderoga set him on the road to political leadership. In Richard Montgomery's ill-fated invasion of Canada, however, he led a force of poorly equipped and trained Canadians, against orders, in a disastrous assault on Montreal that ended with his own capture.

Exchanged after two and a half years, he returned

to a Vermont functioning under a newly adopted constitution featuring a unicameral legislature and elected judges. Although he held no elected office, he and his Green Mountain Boys maneuvered their adherents into positions of power. Confiscated Loyalist property provided a means to peddle influence with gifts to members of the Continental Congress and officers of the Continental army. Although Allen's only official position was as commander of the state militia, the assembly delegated to him authority to act as he saw fit. In what Bellesiles sees as "populist strategy" and "personalization of political authority" (pp. 158, 163), the opposition was overwhelmed. New York land titles were declared null and void, while New Hampshire Governor Benning Wentworth's equally dubious titles, bought by a majority of the settlers at low prices, were upheld. Bellesiles refrains from labeling this as rule by the "Arlington Junta," as have Randolph A. Roth in *The Democratic Dilemma: Religion, Reform, and the Social Order in the Connecticut River Valley of Vermont, 1791–1850* (1987) and Charles A. Jellison in *Ethan Allen, Frontier Rebel* (1969).

Bellesiles is also the advocate when he contends that Allen intended no more than an alliance and trade treaty in his negotiations with Frederick Haldimand, governor of Canada, proposing admission of Vermont as a British province. He is on firmer ground when he asserts that Allen used the negotiations to fend off the danger of a British takeover from the north, and at the same time to intimidate the Continental Congress into recognition of Vermont statehood. And at the head of his Green Mountain Boys, he forced New York and New Hampshire to back off from attempts to impose their authority. All this was accomplished without any major military encounter. He broadcast his prowess with publication of a narration of his captivity, which went through eight editions in two years. He saw politics, says Bellesiles, as "primarily the art of theater" (p. 152).

In retirement, Allen devoted himself to his theological treatise, *Reason, the Only Oracle of Man* (1785). As Bellesiles interprets it, Allen rejected the Bible's doctrine of original sin because he saw it as justifying the death of his cherished father, which he refused to accept. Using a "logistical trick" (p. 20), he denied original sin, and therefore the need for redemption and the very reality of death. When he moved to his thousand-acre farm north of Burlington, he brought his father's deathless remains with him. For himself, he expected immortality as the founder of Vermont. In this most extensively documented biography yet produced, Bellesiles masterfully chronicles that achievement.

MAX M. MINTZ

Southern Connecticut State University

JOHN E. CROWLEY. *The Privileges of Independence: Neomercantilism and the American Revolution*. (Early America: History, Context, Culture.) Baltimore, Md.:

Johns Hopkins University Press. 1993. Pp. xiv, 215. \$32.50.

The concept of "neomercantilism" lies at the heart of John E. Crowley's book. Although Crowley never explicitly defines this term, neomercantilism is an outlook that lies somewhere between the orthodoxies of mercantilism and the liberalism of free trade. This is a valuable, informative work, weakened only by poor writing that at times obscures the ideas discussed.

Crowley's book surveys views of political economy in Europe (chiefly Great Britain) and the thirteen colonies/states between about 1750 and 1794. Even at mid-century writers had begun to question the wisdom of government intervention in the marketplace. Montesquieu's *Spirit of the Laws* (1748), for example, demonstrated the "oblique conflict between nascent economic liberalism and mercantilism" (p. 2).

This new outlook emerged more slowly in the thirteen colonies. Before Independence, Americans, Crowley shows, did not fundamentally question mercantilist doctrines. The colonists bitterly resented British taxation but were hesitant to demand free trade. Even during the final crisis of 1774–76 they were reluctant to open their ports to the world.

Meanwhile, leading British thinkers and politicians were becoming increasingly critical of the rigidities of mercantilist thought. Adam Smith is a central figure in this book, and Crowley's reading of Smith is an intriguing one. Crowley argues that Smith was a neomercantilist rather than a free trader. He notes that Smith did not call for an abolition of the Acts of Trade but rather desired (in Smith's words) a "moderate and gradual relaxation" of those measures (p. 92). And he underscores Smith's support for the Corn Laws.

Smith influenced a generation of British leaders, and these individuals, Crowley observes, criticized various aspects of the Navigation Acts. Among these writers and statesmen were Charles Jenkinson (later Lord Hawkesbury), Lord Sheffield, and George Chalmers. Crowley explores the views of these men, and others, and their efforts to shape relations with the newly independent United States.

The chapters on the new American nation focus chiefly on James Madison, Alexander Hamilton, and Thomas Jefferson. Madison, in Crowley's view, was an old-fashioned mercantilist who occasionally talked about freeing trade. He believed in retaliatory commercial legislation and extensive government intervention. Hamilton, by contrast, was the "preeminent neomercantilist in the United States" (p. 153). Hamilton included lengthy paraphrases of Smith's *Wealth of Nations* (1776) in his work but nevertheless accepted the need to protect American industry. Jefferson's views resembled those of Madison.

Crowley's use of neomercantilism casts new light on the ideologies of this era. This book helps further unravel the republican synthesis of the 1960s, which

emphasized political ideas and brushed aside questions of economic interest. Crowley's work thus reinforces and extends the insights of other historians—such as William A. Williams, Drew McCoy, and Joyce Appleby—who have written on the political economy of the late eighteenth century.

I must report, however, that this is not an easy book to read. The prose is often dense, and the larger themes frequently disappear in the detailed consideration of one writer or politician. The book would benefit from a true introduction and conclusion. Still, I would encourage readers to tackle this work. It is a thoughtful essay and sharpens our understanding of the revolutionary era.

MARC EGNAL
York University
Toronto

DAVID B. POTTS. *Wesleyan University, 1831–1910: Collegiate Enterprise in New England*. New Haven: Yale University Press. 1992. Pp. xvii, 383. \$35.00.

Many academic historians have attempted to write histories of their institutions in relation to the educational, social, and religious events of the period, but few have succeeded as well as David B. Potts in his history of Wesleyan University. Synthesizing voluminous archival resources, he tells the story of the years from the founding of the institution in 1831 as a small Methodist college serving the sectarian needs of a rural, agrarian nation to its emergence as a first-rate liberal arts college in the secular, urban society of 1910.

Wesleyan University was created primarily by the citizens of Middletown, Connecticut, who provided land, buildings, and pledges, in conjunction with the Methodist clergy of New York and New England. As an evangelical Protestant college committed to a liberal arts education, Wesleyan prepared its students to become clergymen and educators devoted to lives of service.

By mid-century the community of Middletown played the role more of a lender than a donor with the result that the Methodist church assumed greater responsibility for the institution. The original charter of 1831 was amended in 1870 to provide that a majority of the trustees, the president, and a majority of the faculty be members of the Methodist church. By the turn of the century Wesleyan was identified as a denominational college. In 1872 the college, following the lead of other institutions, admitted women.

But clerical control did not endure. Between 1870 and 1910 the faculty gained professional independence as a result of the new elective curriculum, department reorganization, new methods of teaching, and support facilities. These changes threatened to diminish Methodist piety by creating a gap between professional values held by Wesleyan's faculty and the beliefs within the Methodist church.

By the early years of this century, alumni feared the

sectarian image repelled more students than it attracted—causing Wesleyan to fall behind the other New England colleges with regard to enrollment, endowment, athletics, public image, and leadership. As a result the charter was again revised in 1907 to provide that board members, the president, and faculty members did not have to be members of the Methodist church. Fearing the “feminization” of the university as the enrollment of women rose to 25 percent, the board decided in 1909 it would no longer admit women. These actions cleared the way for the university to become a traditional male New England liberal arts college of high reputation.

Potts writes largely from the point of view of the board and the alumni, for which he had extensive records, although he also includes lengthy descriptions of student life. His meticulous research and careful analytical writing was made possible by generous support from the university for a four-year period. He sets the entire story of Wesleyan in the context of New England higher education by frequent comparison with Amherst and Williams. This book is a contribution to the history of American higher education. One hopes that a second volume will soon follow.

MARY MARTHA THOMAS
Jacksonville State University

THOMAS W. CUTRER. *Ben McCulloch and the Frontier Military Tradition*. (Civil War America.) Chapel Hill: University of North Carolina Press. 1993. Pp. 402. \$34.95.

Ben McCulloch, although little known outside of Texas today, led one of the most eventful lives in antebellum America. He came to Texas as a teenager following his illustrious neighbor, Davy Crockett. A fortuitous illness saved him from Crockett's fate at the Alamo, but he went on to fight with Sam Houston at San Jacinto. He became Houston's protégé and served him as captain of Texas Rangers. He was already famous on the frontier for his ranger adventures when General Zachary Taylor picked him as chief of scouts for his campaign into northern Mexico during the Mexican War. This gave McCulloch considerable national renown, which increased with his stint as a lawman during the California gold rush, a U.S. marshal in Texas, and a peace commissioner to Utah during the Mormon War of 1857. He then played a key role in leading Texas out of the Union, became the first civilian commissioned a general in the Confederate Army, commanded the rebel forces in the West, and died in battle at Pea Ridge, Arkansas, in March 1862. All of this is ably chronicled by Thomas W. Cutrer in a fast-paced, clearly written narrative that does full justice to its heroically oversized subject.

Cutrer frames his biography around the theme of the antebellum conflict between the rise of a professional West Point-trained officer corps and the won-

derfully American, self-taught military tradition out of which McCulloch came. Despite his well-deserved military fame McCulloch's repeated efforts to gain a commission in the regular U.S. Army were blocked by the new military establishment that emerged from the Mexican War. Jefferson Davis, in particular, was McCulloch's nemesis in this quest for position and it is particularly ironic that as president of the Confederate States of America he would again control McCulloch's fate during the Civil War. Davis's misgivings about McCulloch's ranger mentality, both his greatest strength and weakness, were proven correct when the general engaged in his own scouting at Pea Ridge, was promptly blown out of the saddle by Yankee sharpshooters, and thus contributed greatly to a crucial rebel defeat by his act of reckless courage.

This biography is a volume in Gary Gallagher's "Civil War America" series and so it is not surprising that seven of its fifteen chapters are concerned with the war. These chapters display a misplaced sympathy for the "lost cause" that will irritate many readers and they, like me, will find the chapters on McCulloch's more fascinating and significant frontier career of far greater interest. It is in those chapters that Cutrer, like his subject, is best able to really shine.

PAUL ANDREW HUTTON
University of New Mexico

MARK E. NEELY, JR. *The Last Best Hope of Earth: Abraham Lincoln and the Promise of America*. Cambridge: Harvard University Press. 1993. Pp. viii, 214. \$24.95.

There have been so many books written about Abraham Lincoln that one is tempted to groan at the appearance of yet another one. In the case of Mark E. Neely, Jr.'s concise study, however, smiles and applause are in order. The sole composer of *The Abraham Lincoln Encyclopedia* (1982) and the author of a prize-winning examination of Lincoln and civil liberties during the war, *The Fate of Liberty* (1991), Neely is widely recognized as one of the foremost authorities on Lincoln. It is with skilled pen (or word processor) and keen mind that he now presents this volume, which marks the opening of an extensive Lincoln exhibit at the Huntington Library. General readers seeking an introduction to Lincoln can not do better than to turn to it; scholars of Lincoln and his times will find it full of ideas to gnaw on.

More a collection of essays than a true biography, this volume nevertheless provides an insightful introduction to key themes in Lincoln's public life. Neely is far more interested in the public than the private Lincoln: Ann Rutledge is mentioned once in passing, and Mary Todd Lincoln remains far in the background. Neither does he trouble himself with extended examinations of Lincoln's psyche or soul. Rather, he chooses to look at Lincoln the politician and president, reminding us that it was because of these pursuits that we remember Lincoln today. Two chapters trace Lincoln's path in politics from a fron-

tier Whig to Republican presidential candidate and include an incisive summary of Lincoln's perspectives on slavery and race during these years. The bulk of the book, however, covers Lincoln's presidency. Most rewarding are the chapters on the northern home front and politics, revealing as they do Lincoln's iron hand and velvet glove. When it came to justifying conscription legislation or the arrest of Clement Vallandigham, Lincoln, according to Neely, was not nearly as persuasive or shrewd as he was in other areas. Such assessments reflect Neely's balanced and dispassionate approach to his subject: although he is obviously partial to Lincoln, he notes his shortcomings in several areas.

Here and there Neely's analysis falls short. Many readers will find debatable his claim that Lincoln was unwilling "to allow partisan concerns to interfere with decisions critical to the army" (p. 90). Lincoln's support of John A. McClernand's ambitions in 1862 and the retention of Nathaniel Banks and Franz Sigel in important commands at the outset of the campaigns of 1864 were clearly colored by politics, and they carried military costs, as did the president's reluctance to endorse Ulysses S. Grant's desire to remove Benjamin F. Butler in the summer of 1864. Indeed, Neely's treatment of Lincoln as commander in chief is somewhat disappointing, especially in its rather traditional interpretation of the relationships between Lincoln and his generals. His discussion of Lincoln and emancipation is thoughtful if unexceptionable, demonstrating both the growth and limits of Lincoln's vision as he moved carefully toward adopting emancipation as a war aim.

Enhanced by a selection of illustrations drawn from the Huntington exhibit, Neely's book offers a succinct and provocative understanding of this most-studied president. This is in itself a remarkable accomplishment: one hopes that someday Neely chooses to expand on this promising beginning.

BROOKS D. SIMPSON
Arizona State University

J. DAVID GREENSTONE. *The Lincoln Persuasion: Remaking American Liberalism*. (Princeton Studies in American Politics: Historical, International, and Comparative Perspectives.) Princeton: Princeton University Press. 1993. Pp. xxxiii, 312. \$24.95.

In the tradition of works on Abraham Lincoln's political thought by such authors as Garry Wills and Harry Jaffa, J. David Greenstone accords to the Illinois railsplitter a central place in the reformulation of American political thought. Drawing inspiration from Marvin Meyers's portrait of the Jacksonian persuasion, and informed by Wittgensteinian categories of analysis that stress the constitutive grammar of politics over the causal or predictive nature of historical discourses, Greenstone argues that Lincoln built on the flawed legacy of the "silver generation" of Henry Clay, Daniel Webster, Andrew Jackson, and Martin

Van Buren to resolve the central dilemmas of American liberalism during the Civil War era. The author argues that although "at its core American political culture is pervasively liberal" American liberalism is itself divided between "reform liberals" who seek "to develop the faculties of individuals" and "humanist liberals" who "were concerned primarily with the satisfaction of the preferences of individuals" (p. 6). The conflict between these outlooks emerged in the 1830s, when Lincoln was becoming politically active, and exploded over slavery during the 1850s and 1860s.

Two issues left unresolved by the founding generation (how to build stable electoral majorities in a pluralist nation, and how to reconcile the claims of morality with the necessity for compromise) confounded humanist liberals such as Van Buren (whose answer was the party system) and reform liberals like John Adams (who subsumed antislavery to devotion to the Union). Through textual analysis of the public utterances of pairs of major politicians and reformers in the antebellum era, Greenstone investigates the second generation's divisions over these questions. Lincoln's achievement was to connect the individualism of humanist liberalism to the communal outlook of reform liberalism when he made emancipation a condition of reunion. In the end, "the Lincoln persuasion was a very successful synthesis of both outlooks; and it provided the North with a politically viable and intellectually coherent stand on slavery, because it added humanist features to a more complex and subtle reform liberal understanding of the issue" (p. 7). In essence, Lincoln accomplished what Louis Hartz thought had already been done decades before in uniting the discordant strands of American liberalism into a broad, diffuse, and stultifying persuasion that has yet to be dislodged.

This is a complex, dense book that is preoccupied with classifying, defining, and analyzing the grammar of politics, almost disembodied from the personal ambitions and tactical challenges that drove politics and policy in the early republic. Its conclusion that the "bipolarity" of American liberalism ended with Lincoln does not address the alternative Americas proposed by socialists, conservatives, labor activists, or suffrage movements (presumably because the latter were liberals appropriating rights talk on behalf of their campaign for admission to the public square). Greenstone's death left the completion of the manuscript to his students and colleagues, who provide an excellent introduction about Greenstone's method and the derivation of his arguments.

PETER KNUPFER
Kansas State University

REID MITCHELL. *The Vacant Chair: The Northern Soldier Leaves Home*. New York: Oxford University Press. 1993. Pp. xiv, 201. \$25.00.

In writing this book, his second on Civil War soldiers, Reid Mitchell provides an insightful look into the mind of nineteenth-century America. His earlier work, *Civil War Soldiers* (1988), dealt with men on both sides; this work concentrates on the North and attempts to explain how "the centrality of home and the family to northern culture made them central to the northern soldier's understanding of the Civil War" (p. xiii). Mitchell illustrates this fusion of war, home, and family in a variety of ways. He contends that although northern men understood the basic ideological issues involved in the war, they also regarded the Union as a kind of extended family that tied them to both generations past and generations yet to come, and they were obligated to preserve that family. In this metaphor, they viewed southerners as wayward children who had rebelled against parental government and against God, the ultimate Father. The rebels had to be punished, but then welcomed back into the family, a reunification symbolized by the marriage of northern veterans with southern belles. Northerners also viewed the Union army, and especially their regiment, as an extension of their community, an impression reinforced by constant communication between home and battlefield. Their military unit became a substitute family in which officers assumed paternal roles over their "boys," helping them make the transition from youth to manhood and citizenship.

Mitchell is most insightful in searching for soldiers' ideas about gender. War, as always, made men out of boys, and Mitchell explores what becoming a man meant to the young recruits. At the same time, he discusses concepts of femininity and shows how soldiers attempted to make up for the absence of women, particularly mothers, in their lives. Nurses provided some of the nurturance the men craved, but their officers, in addition to providing paternal guidance, also supplied some emotional support. Mitchell has a revealing analysis of the complex reaction of northern soldiers to southern women, who in their behavior seemed to violate most concepts of proper femininity. His explanation for why northern male anger toward southern white women rarely took the form of rape is quite provocative.

In Mitchell's earlier work he contended that northern and southern soldiers interpreted the world in which they lived in much the same way. Here he suggests that if both sides fought in defense of community values, their war was over "different concepts . . . of what this American home was" (p. 37). Because this book is about northern soldiers, Mitchell does not develop this thought. He does point out, however, that whereas men on both sides sought to combine their roles as sons, husbands, citizens, and soldiers, the resulting fusion strengthened the northern war effort while weakening the South's, for by the last year of the war Confederates were leaving the army to protect their families. Thanks to Mitchell's two books, and recent works by Michael Barton,

Gerald Linderman, Randall Jimerson, Earl Hess, James Robertson, and James McPherson, we know much more about the world of the Civil War soldier. The time has come for a comprehensive treatment of the subject that integrates the information and insights provided by these historians. McPherson has one such study under way; certainly Mitchell has established his credentials to produce one as well.

RICHARD H. ABBOTT
Eastern Michigan University

ROBERT M. BROWNING, JR. *From Cape Charles to Cape Fear: The North Atlantic Blockading Squadron during the Civil War*. Tuscaloosa: University of Alabama Press. 1993. Pp. xi, 453. \$44.95.

Robert M. Browning, Jr., correctly observes that naval operations have been slighted in Civil War literature. "This lack of scholarship is surprising," he adds, "since . . . the navy performed numerous roles and missions that were crucial to Union victory" (p. x). Browning partially remedies this neglect with his fine study of the North Atlantic Blockading Squadron, a component of the U.S. Navy created in September 1861 to operate along the coasts and on the inland waters of Virginia and North Carolina.

Browning's analytical work, the first on any Civil War naval squadron, covers considerable ground. It assesses the principal Union officers; explores the squadron's blockading activities, participation in combined operations with the army, and efforts to destroy Confederate naval forces in Virginia and North Carolina; and accords significant attention to the squadron's Confederate opponents. Away from the arena of combat, Browning devotes chapters to how the navy built the fleet through the problematic conversion of commercial vessels and the sometimes hurried construction of new steam-powered warships, supplied it, struggled with a daunting problem of repairs, and sought to create a system for delivering huge amounts of coal. Recruiting became increasingly difficult as the war progressed, especially the enrollment of men familiar with steam technology. "The enlistment of blacks, probably more than any other factor," states Browning, "helped to alleviate the shortage of men" (p. 203). Eventually comprising approximately 15 percent of the squadron, African Americans served alongside white sailors but "usually had the dirtiest jobs or worked on board the supply hulks of the squadron" (p. 204).

The squadron receives generally high marks from Browning, who concludes that it "played a major role in the outcome of the war" (p. x). Although it never achieved an airtight blockade, its presence severely limited the amount of material coming into the Confederacy through North Carolina. "More significant was the Union navy's control of the ocean and inland waters" (p. 403), which disrupted Confederate transportation along its coasts and rivers (thereby complicating Robert E. Lee's logistical situation in

Virginia) and tied down thousands of rebel troops. The failure of army commanders to cooperate satisfactorily with the navy compromised the Union war effort in the eastern theater. Only U. S. Grant made full use of the navy to guard communications and bases of supply, transport soldiers, and support vulnerable positions along the James River. The army and navy mounted perhaps their best combined effort in the final attack against Fort Fisher and the capture of Wilmington, North Carolina, in early 1865.

Browning's focus on the navy sometimes leads him to distort the strategic situation in Virginia. For example, he criticizes commanders of the Army of the Potomac who "failed to use the navy and the advantages that control of the water could give them" and "likewise failed repeatedly to capture the rebel capital" (p. 60). This overlooks the facts that Abraham Lincoln consistently urged his generals to make Lee's army rather than Richmond their target, and that many of the best opportunities to harm the rebel force lay beyond the reach of naval support.

Such lapses easily may be forgiven Browning, whose book is impressively researched in manuscript sources and well presented. This study sets a standard that other historians of the navy's part in the Civil War would do well to match.

GARY W. GALLAGHER
Pennsylvania State University

WINTHROP D. JORDAN. *Tumult and Silence at Second Creek: An Inquiry into a Civil War Slave Conspiracy*. Baton Rouge: Louisiana State University Press. 1993. Pp. xvii, 391. \$24.95.

In the spring of 1861 a small group of slaves who had gathered around Second Creek in Adams County, Mississippi, began talking about the war between North and South, the abolitionist tendencies of President Abraham Lincoln, and their own aspirations for freedom. In the days that followed, at one place or another, more slaves from more plantations joined the discussion that included killing masters and raping women and meeting up with the Federal army, which the rebel slaves were certain would soon capture New Orleans and head toward Natchez. The slave conspiracy that began in the spring at Second Creek was over by the fall of that same year, ending at the Natchez fair grounds with the hanging of at least twenty-seven slaves, all without a mention or a whisper in the press.

Winthrop D. Jordan's narrative spins outward from an extraordinary document, a sometimes cryptic and admittedly incomplete transcript that planter Lemuel P. Conner kept while the accused conspirators were interrogated by an "Examination Committee," which had been called together for that anxious purpose. Through an exacting search for additional information, Jordan uncovered bits and pieces of the story in private letters and diary entries, military correspon-

dence and Southern Claims Commission Reports, and even from a former slave interviewed by the WPA.

These materials provided the base for the book, which can be described as operating at three levels: one explores Adams County plantation life and society, another examines selective aspects of slave life in the antebellum South, and the third details the conspiracy itself. Jordan has done a tremendous job explaining and defining the planters and the plantation world that gave rise to the conspiracy. There is also a great deal of thoughtful information about life in slavery that shaped the rebels' plot for freedom and, in some cases, revenge. Those two overlays offer rich glimpses and keen insights of plantations and planters, slaves and slavery, ideas and values, crops and hard labor, religion and ideologies, and most certainly crime and punishment. Yet when those overlays are lifted off, the plot and the lives of the conspirators seems tantalizingly incomplete, particularly by comparison.

There is so much left unknown. How many were involved; how specific was the plan; how would they carry it out; what did they know about the fighting; how many were questioned; were they tortured; how many hanged; how many left in irons; what about their families; and on it goes with natural historical curiosity.

The conspiracy documents drive the book (and are reprinted as appendixes), but unlike most other slave conspiracies, the one at Second Creek was never made public, and understandably so. It was wartime. Would anyone in the Confederacy have wanted the public—South or North—to get its hands on Conner's transcript that recorded slaves speaking of rising up, killing masters, "riding" mistresses, and seeking out the Union Army as allies? The South's demand for silence about restless slaves helped to preserve the historical integrity of the documentation.

So while the account of the conspiracy seems incomplete, the private nature of the documents makes the saga feel genuine and immediate, even intimate, made more so by the harsh language and bold claims of black men who must have known they were on the way to the gallows.

What Jordan brings forth, in more subtlety and detail than space allows to examine here, is the complexity of slave life, of contradictions and ambiguities—both black and white—over loyalty and betrayal, trust and violence, sex and domination, freedom and bondage, oppression and resistance, paternalism and independence, and life and death in the slave South.

This book is both a fascinating and a frustrating study, fascinating for what Jordan is able to wring out of a small handful of skimpy documents, and frustrating for what he is unable to explain because history would surrender nothing further, even to his skilled hands.

C. PETER RIPLEY
Florida State University

R. B. ROSENBERG. *Living Monuments: Confederate Soldiers' Homes in the New South*. Chapel Hill: University of North Carolina Press. 1993. Pp. xiii, 240.

Some historians have argued that the "Cult of the Lost Cause" helped the white South reintegrate itself into the nation's economy and culture after 1865; other historians have argued that the reverence for the Confederacy was a genuinely reactionary force in the South, opposed to reconciliation with the Yankees and modernity. R. B. Rosenberg examines this issue from the ground up. We do not read here of evanescent rhetoric or ceremonies, but of bricks and mortar, legislature and central office, aging men and a forgetful society. The Lost Cause looks different because of this book.

Rosenburg casts a critical eye on the gap between word and deed, ideal and practice. He chronicles the disregard, resentment, and impatience with which the New South treated ex-Confederates when it was not extolling their wartime valor. The homes, despite the rhetoric and hopes with which they were launched, quickly became depressing places where elderly men waited to die. Legislative wrangling, editorial wars, foot-dragging, and grudging appropriations preceded the opening of the homes. The new buildings filled with veterans whose wounds and amputations had never healed, whose ability to support themselves had long since passed, whose families were dead or neglectful. Regimentation and surveillance hounded the inmates, who spent much of their time furtively drinking or dulling themselves with patent medicines.

Rosenburg details the grim struggle between these somewhat ghostly veterans and their keepers. He has gathered what systematic information there is, tallying the characteristics of those admitted and those expelled, the enrollments and the expenses. He presents haunting photographs and bits of poetry written by the inmates, testifying to their despair and cynicism. He tries to account for the dates the homes were opened and closed, tries to discern their larger meanings. What Rosenberg finds may not be startling, but the cumulative effect of his chapters is powerful.

Ironically, the more we study the symbols of southern distinctiveness, such as these Confederate veterans' homes, the more we see how much the postwar South shared with the North. As with veterans' organizations and their various auxiliaries, the South actually fell behind the North in expressing its respect and reverence for the soldiers of the past. The major difference was that the victorious North enjoyed the luxuries of victory and a bulging federal treasury, whereas the South had to piece together its "cult" from defeat and state funding. Rosenberg shows us how little concrete reward the soldiers of the Confederacy received once the guns stopped and the rush into the future began.

EDWARD L. AYERS
University of Virginia

MIRIAM FORMANEK-BRUNELL. *Made to Play House: Dolls and the Commercialization of American Girlhood, 1830–1930*. New Haven: Yale University Press. 1993. Pp. xi, 233. \$25.00.

There probably is not a woman alive in this country who does not have memories of doll play. Despite the universality of that experience, however, Miriam Formanek-Brunell's book is the first to focus on the significance of dolls in American culture.

Using sources such as periodicals, catalogues, memoirs, and patent applications, Formanek-Brunell argues that the doll culture that emerged in the early nineteenth century was characterized by three struggles. The first was a struggle between adults and children over what meaning a doll should have in a child's life. For adults, dolls provided an opportunity to idealize children, to inculcate middle-class values as well as social and domestic skills in children, and to commercialize childhood.

Despite adult attempts to manipulate children, Formanek-Brunell argues that girls were active rather than passive consumers of dolls. She suggests that girls often ignored their dolls and spent most of their leisure time engaged in physical, outdoor activities. Moreover, when they did play with dolls, their play was characterized by aggressive as well as more socially acceptable behavior. The author's case for childhood agency, which is based on limited anecdotal evidence, is class-specific since girls from working-class families often had neither the money to spend on dolls nor the time for doll play.

The second struggle took place between American and European doll manufacturers. Throughout the nineteenth century, American parents preferred to buy fragile and elaborately dressed imported dolls rather than those produced in the United States. World War I marked the end of European domination of the doll market. The embargo of German doll parts and anti-German propaganda campaigns combined with a postwar tariff allowed American doll manufacturers eventually to eliminate their European competition.

The third struggle took place between American male and female doll manufacturers to determine the kind of dolls that would be produced and the kind of environment in which their manufacture would take place. Men produced dolls that were technologically innovative, glued together, and intended to amuse and entertain. They manufactured dolls in factories where workers found it difficult to establish any sense of community. The goal for early female doll manufacturers was to produce dolls that were safe, portable, soft, durable, sanitary, and realistic. Their dolls were sewn together and were intended to teach children about relationships. Women established a cottage industry characterized by a sense of community and pride in the skill that was required to produce a doll. The male model of production prevailed, however, and by the 1930s there were no

longer significant differences between male and female doll manufacturers.

The author describes the production of dolls intended for boys as well as girls but neglects the kind of complete comparative analysis that would address larger issues of gender differentiation. Nevertheless, the book makes a solid contribution to the literature on childhood as well as business history and, like Karin Calvert's *Children in the House* (1992), illustrates the use that can be made of material culture in historical research.

SYLVIA D. HOFFERT

Southwest Missouri State University

JERRY GRISWOLD. *Audacious Kids: Coming of Age in America's Classic Children's Books*. New York: Oxford University Press. 1992. Pp. xiv, 285. \$25.00.

This study by Jerry Griswold is boldly audacious, compelling, and convincing in its argument. Griswold defines "The Golden Age of Children's Books" as the period between the Civil War and World War I, noting that the dozen volumes that he analyzes were all best sellers during that period.

Griswold finds a pattern that threads its way through the children's books of this period. Oversimplified, the pattern is the story of an orphan, who suffers from poverty and neglect and who is frequently "dispossessed royalty" or at least remembers a "vanished happy time" that existed prior to the beginning of the plot of the novel. Often the child's parents' marriage was unacceptable to society, and as a result of social disapproval the child takes a journey to another life.

The destination of that journey is either "the Big House or the Great Outdoors," where the child is adopted by a second family of a dramatically different social rank from the youthful voyager. There is usually a villain of the same sex as the child and an older helper of the opposite sex. The child becomes a hero, heroine, or savior and his or her identity is recognized, and is accompanied by "wealth, inheritances, the return of the Vanished Happy Time" (p. 9). Finally, "some accommodation must be made between the child-hero's two lives. Some . . . choose between the two . . . Other characters integrate their two lives . . . Still other stories conclude on the tragic note of their child-heroes' inability to integrate their two lives" (p. 9).

This may sound too formulaic, but that is not the case. Griswold amasses persuasive evidence from a number of disciplines to suggest exactly why such a pattern should have been compelling to American children's literature of the period: psychologically, socially, and historically these fictions catered to a unique American psyche. Issues of identity paralleled the trope of "America-as-Child," in revolt against a parent or authority symbol. "It is not surprising," Griswold argues, "that, among a variety of other options, the pervasive pattern that emerged was one

of an orphan, estranged and engaged in oedipal rebellion against its parents, making its Declaration of Independence and achieving autonomy. In it we hear an echo of our own national endeavors" (p. 16). In addition, Griswold integrates the pattern of children's literature into significant issues of popular culture of the period: a preoccupation with health, nostalgia, and naïveté; new theories of child-rearing; and popular images of the child as public figure "as a vehicle for nostalgia or as a symbol of the future's promise" (p. 24).

What follows the generalizations is a sophisticated analysis of the twelve novels: *The Wizard of Oz*, *Adventures of Huckleberry Finn*, *Rebecca of Sunnybrook Farm*, *Little Lord Fauntleroy*, *Tarzan of the Apes*, *The Prince and the Pauper*, *The Adventures of Tom Sawyer*, *Little Women*, *Toby Tyler*, *Hans Brinker*, *The Secret Garden*, and *Pollyanna*. Obviously, not one of the novels satisfies the entire psychological pattern that Griswold has deduced. But the interpretations of each book are lively, original, and persuasive.

It is possible to quibble with some of Griswold's readings and generalizations: when he concludes that his dozen children "are all audacious kids who refuse to be vanquished" (p. 236), he seems to ignore some of the darker implications of his own analyses, oedipal longings for parricide that come true, repressions successfully imposed by adults, and deterministic limitations imposed by heredity and environment. Nevertheless, this book is felicitously written and provocatively argued, truly a significant contribution to the study of American literature and culture.

HAMLIN HILL
Texas A&M University

C. WINSTON CHRISLOCK. *Charles Jonas (1840–1896): Czech National Liberal, Wisconsin Bourbon Democrat*. Cranbury, N.J.: Associated University Presses and the Balch Institute Press, Philadelphia. 1993. Pp. 209.

Charles Jonas was a Czech-American journalist and Wisconsin Democratic politician who emigrated to the United States in 1863 but never fully assimilated into his adopted country. His activities in the United States were often conceived with an eye toward their repercussions in his native Bohemia. Divided loyalties and enduring attachments to their homelands were not uncommon among nineteenth-century immigrants, but few incarnated and exploited these qualities in the Czech community as transparently as Jonas.

As C. Winston Chrislock relates in this first biography of Jonas in English, the articles that Jonas wrote in *Slavie*, a Czech-language weekly that he edited in Racine for over thirty years, influenced politics and public opinion in the Midwest and well beyond. He wrote Czech grammars and bilingual dictionaries that long held pride of place on library shelves. Elected a Wisconsin state senator as a Democrat in 1883, he served twice as lieutenant governor from 1891 to 1895

and was twice appointed by President Grover Cleveland to be United States consul to Prague and once to St. Petersburg. He married the daughter of *Slavie's* co-owner, a union that, despite difficulties he had in "fulfilling his marital obligations," produced four children and "had the appearance of a normal family life" (p. 42). Chrislock tantalizes the reader with this and other cryptic comments about Jonas's private life but does not discuss their possible connections with his public persona and tragic end.

He was born Karel Jonáš in Habsburg Austria, where as a youth he imbibed the prevailing Czech liberal nationalism, pan-Slavic yearnings, freethinking, and dislike of centralized arbitrary rule of the decade following the failed revolution of 1848. In 1860, he published an anti-German tract, took part in a student demonstration, and was jailed and banished from Prague. Chafing at these restraints and facing possible military service, Jonas departed for England and then the United States. He retained lifelong contacts with upper-class liberals in Prague such as the renowned patrons of Czech culture Vojta Náprstek and his wife Josefina. In the United States, journalism and politics became his vehicles for advancement.

Chrislock brings out that Jonas's youthful transgressions and his exhortations in *Slavie* for Austria to reform as a federal state with civil liberties on the American model were duly recorded by the Austrian police. As a Wisconsin "Bourbon" Democrat, he opposed nativism, prohibition, paper money, and strong central government. Chrislock is especially enlightening on Jonas's ties with the U.S. Senator from Wisconsin, William Freeman Vilas, who was his stalwart supporter. Vilas helped persuade Cleveland to appoint Jonas as consul to Prague in 1885, but the Austrians delayed accepting him for a year and a half. During that interval, "his political career and family life began to deteriorate" (p. 124). He was routinely dismissed after Cleveland's loss to Benjamin Harrison and apparently was thereafter consumed by a desire to return to Prague. While his career in Wisconsin reached new heights, inwardly "his whole spiritual psyche collapsed" (p. 131). He was renamed consul to Prague in 1895 during Cleveland's second term, but this time the Austrians flatly rejected him, and "Jonas's world was shattered" (p. 164). He committed suicide in Germany in 1896.

The strength of Chrislock's book derives from his diligent use of Austrian police reports, materials in Czech archives, and newspapers, letters, and private papers in the United States. He relies on studies by Josef Polišenský and Zdeněk Šolle, among other Czech scholars, and works on Wisconsin politics. The book leaves unanswered questions about Jonas's emotional stability, thirst for recognition, finances, life style, and family, particularly when he worked in Europe and his family often remained in Racine. The pedestrian narrative is burdened with summaries of letters and speeches, and 802 reference notes to 172

pages make for tedious reading. Among numerous typographical and factual errors, one notes a reference to *Národní* instead of *Národný* (p. 58) and Edward Taaffe instead of Taafe (p. 115), while Karel Kramář is linked to neo-Slavism in the mid-1890s (p. 154), more than a decade before the movement originated. Nevertheless, Chrislock has advanced our knowledge about a neglected figure in Czech and American history.

STANLEY B. WINTERS
New Jersey Institute of Technology

ALAN LESOFF. *The Nation and Its City: Politics, "Corruption," and Progress in Washington, D.C., 1861–1902*. Baltimore: Johns Hopkins University Press. 1994. Pp. xii, 337. \$45.00.

The nation's capitol stands apart from other major U.S. cities in at least two key respects: industry played only a minor role in urban growth, and after 1874 an appointed commission administered local government. Nevertheless, Alan Lessoff finds that Washington decision making reflected a key element found elsewhere: a "promotional tendency," a "mix of attitudes, laws, and administrative practices, which was ubiquitous in this country's public sphere in the last century" that "made our governments . . . proficient at fostering economic growth but less adept at channeling that growth towards shared public purposes" (p. 273). From 1850, "government interacted with the private sector primarily as a promoter, a prime mover rather than shaper of society" (p. 70). Lessoff traces the capitol's retreat from democratic politics, the promotional tendency and its perpetrators, the "improvers," through the Territorial Government (1871–74), and the District Commission to 1902.

Within this larger framework Lessoff carefully documents the key groups that played major roles in the District's decision making. Territorial governor "Boss" Alexander Shepherd, a "Napoleon of sewer pipe and pavement," along with his band of "improver Republican" contractors, developers, and businessmen, committed Washington to a massive program of internal improvements (pp. 44, 46). The Citizens' Association, an older, more prominent Democratic elite, opposed this new "emerging civic elite"; both groups embraced the promotional goals to improve Washington, especially its downtown. Under the presidentially appointed Commission government, the Senate District Committee, the Washington Board of Trade, and the Army Corp of Engineers continued these public works aimed at making Washington a monumental city. Lessoff analyzes the make-up of these various groups and effectively traces the shifting alliances that came together to modernize and elaborate on the urban landscape. He also demonstrates how these groups laid the foundation for the Park Commission's report in 1902, considered the United States' first comprehensive urban plan. Whereas plan-

ning and architectural historians give credit to the plan's consultants, including Daniel Burnham and Frederick Law Olmsted, Jr., Lessoff correctly notes the important work of the Board of Trade, the Senate Committee, and others that set the context for those consultants.

This book is extensively researched; it draws on congressional, District Commission, and Army Corps of Engineer records, as well as those of the key individual actors. Lessoff also places his story in a broad comparative framework of U.S. and European urban history.

This is an excellent study; the exposition of the "promotional tendency" in urban government is important both for understanding the past as well as current practices throughout the United States. Nevertheless, there are some problems. Close analysis is a strength of the study, but the detail is occasionally excessive. Lessoff concludes that "most Washingtonians adapted readily and completely" to the commission government (p. 276), with little evidence to support this claim. The book effectively traces elites but seldom penetrates the silence of ordinary Washingtonians. This is especially true for African Americans, who represented one-third of the city's population. Finally, the book does not pursue the consequences of these policies for other aspects of urban life.

JAMES BORCHERT
Cleveland State University

KEVIN T. MCGUIRE. *The Supreme Court Bar: Legal Elites in the Washington Community*. (Constitutionalism and Democracy.) Charlottesville: University Press of Virginia. 1993. Pp. xii, 254. \$40.00.

The significance of attorneys as bridges to the judicial system and agents of legal change has been a staple of law and society research. What is interesting about this body of research is that it focuses almost exclusively on the trial court stage of the judicial process. This is in contrast to most of the public law literature, which concentrates on appellate courts, most notably the U.S. Supreme Court. Kevin T. McGuire successfully marries the two perspectives by examining the influence of lawyers who file petitions, join cases through *amicus curiae* briefs, and argue cases before the Supreme Court.

McGuire's study, the first systematic analysis of its kind, describes the background and work of these attorneys. McGuire is concerned with the scope of influence of a Washington-based elite of the Supreme Court Bar. The significance of the study lies in his ability to conceptualize the types of attorneys who comprise this bar and analyze their impact. Throughout the study, McGuire establishes a context for analyzing this inner circle, comparing them with an outer circle of the bar that exists outside the nation's capital and with the universe of all lawyers.

McGuire begins with a historical analysis of the

Supreme Court Bar, examining the significance of past legal giants such as Daniel Webster and William Pinkney, thus providing a context for the rest of the study. The historical analysis is reminiscent of James Sterling Young's *The Washington Community*. In addition to a rich set of anecdotes, McGuire used a mail survey and interviews with members of the Supreme Court Bar, as well as an exhaustive analysis of cases in the 1977–82 terms. The result is a readable, methodologically sophisticated study of an inner elite and a more peripheral group of attorneys who appear before the Court. This is an important study that opens a new area of research and adds to the existing literature on the agenda and merits stages of Supreme Court decision making.

If there is one flaw in the book, it is that McGuire is too modest in stating the significance of some of his findings. Students of the Court have typically ignored the importance of the written briefs and oral arguments before the Court. Although he finds that the inner circle has a clear impact at the certiorari stage, at the merits stage he underestimates its importance. McGuire accepts the conventional wisdom that good oral arguments cannot win cases, but bad ones can lose them. The literature's myopic concern with the outcomes of cases (who wins and loses) has often diverted attention from these actors and this stage of the process.

The arguments offered in the written and oral briefs of Thomas Emerson in the privacy cases, American Civil Liberties Union and Legal Defense Fund attorneys in numerous cases, and Robert Bork in death penalty cases were adopted in large measure by the Court in its opinions. It would be surprising if the inner elite did not have a similar impact in some landmark decisions or in the evolution of doctrine. Even granting the unlikely premise that oral arguments and written briefs cannot win cases, the skill of litigants can change a vote leading to a stronger or weaker majority and influence opinion language that might yield a broader or narrower final opinion.

RICHARD L. PACELE, JR.
University of Missouri,
St. Louis

MEL WATKINS. *On the Real Side: Laughing, Lying, and Signifying—The Underground Tradition of African-American Humor that Transformed American Culture, from Slavery to Richard Pryor*. New York: Simon and Schuster. 1994. Pp. 652. \$25.00.

This book, testifying to Mel Watkins's deeply felt ambition, contains fully two dozen words in its title. Watkins's hope was to bring to this work the skills earned in a career that included editing *The New York Times Book Review* and writing for a range of serials that ran from *Penthouse* to *The Southern Review*. His goal was no less than tracing a history of African-American humor from its roots in slavery as a subversive—indeed “underground”—heritage to recent

times in the era of the star-crossed stand-up comedian, Richard Pryor.

At his best Watkins largely accomplishes this goal for the general reader on whom the niceties of the theories of cultural studies might be lost, or at least not missed. Moreover, his conviction that he is tracing an otherwise hidden “underground” thread of history is also leavened by his hope to correct for a general “underestimation” (p. 12) of black humor. The result is a nicely turned out inquiry into a form of humor that owes its meanings to inflection rather than rhetoric, exaggeration rather than reality, and spontaneity rather than “studied thought” (p. 34).

Thus, if Watkins's success is owed to his skills as a journalist and raconteur rather than to the rigor or systematic formulation of his theories, its considerable merit as a narrative will not seem the less in the eyes of the informed generalist. In fact, Watkins's easy style and flowing story of African-American humor might lead a friendly reader to wish that he would have stuck to a more purely folkish line of argument and dropped some of the more institutional, electronic forms of performance art such as movies and broadcasting.

Of all the media of performance art, these more corporate forms originated in circles considerably removed from their roots in black literature and “folklore and street humor.” Their collaborative modes of production and their quest for broad mass audiences drove them toward more universalist themes that carried them away from the centers of black life where the success of black humor depended on, as Watkins argues, whether or not it was “by” blacks rather than merely “about” them. Such distinctions were rendered moot by the factory-like circumstances of the production of Hollywood movies or network “sitcoms.” In this sense, his chapters on Hollywood movies, both silent and sound, and on the black presence in broadcasting oblige him to find bits of black humor clinging to such machine-tooled products as Stepin Fetchit's 20th Century-Fox movies and CBS's *Amos 'n' Andy* series. Even though it has become a truism to credit black performers with stretching and making more of their lines than scripts had intended, the products in which they worked hardly qualified as humor “by” African Americans.

Scholarly readers may wish for sharper distinctions in Watkins's account, as well as a more rigorous grounding in the theoretical literature. Even though he touches on similarities between various ethnic forms of humor, we might ask the author for cleaner lines between black humor and, say, the self-deprecation and pessimism of the humor of Jewish stand-up comics such as Jackie Mason; or between black tall tales and those of American frontiersmen; or between black irony and the bitter wryness of the German humor in Alan Dundes's *Life is Like a Chicken Coop Ladder: A Portrait of German Culture through Folklore* (1984). But that would be asking the author for another sort of book, perhaps on the order of Eric

Lott's *Love and Theft* (1993), an essay on minstrelsy grounded in recent cultural studies theory. Yet one might ask for a bit more intellectual rigor on Watkins's own terms as a journalist. For example, we might ask why vaudeville, an incredibly dense Jewish performing medium, should be taken as an omen of "the blackening of America," or we might expect that Watkins's well-handled portrayal of Joel Chandler Harris's tales of Uncle Remus as a sentimentalized white version of essentially subversive black fables should be grounded in a citation such as B. A. Botkin's pioneering *Lay My Burden Down* (1945) rather than in a lone reference to an interview with the actor Robert Guillaume.

Nevertheless, this book should enjoy a deserved popularity among generalists. There are good endnotes that refer mainly to the secondary literature, a serviceable selected bibliography, and an index, but the publisher allowed the rich trove of available illustrative material to appear only in the form of a frontispiece for each chapter.

THOMAS CRIPPS
Morgan State University

GEORGE R. KNIGHT. *Millennial Fever and the End of the World: A Study of Millerite Adventism*. Boise, Idaho: Pacific Press. 1993. Pp. 384.

October 22, 1994, marked the one hundred and fiftieth anniversary of the "great disappointment" of Millerites in America. On that day thousands of people gathered in small groups, awaiting the physical return of Jesus Christ, the shattering of the world, and the inauguration of the millennial joy under the direct reign of God. George R. Knight's book, a sesquicentennial history of this fascinating mass movement, bridges the chasm between recent and older Millerite histories. On the one hand, it synthesizes Millerite monographs that appeared from 1983 to 1987. Drawing on the work of Ruth Alden Doan, Michael Barkun, David L. Rowe, Clyde E. Hewitt, Ronald L. Numbers, and Jon Butler, Knight chronicles the Millerite movement from a national perspective, something these scholars did not intend to accomplish. On the other hand, Knight extends the story of Millerism beyond the work of his predecessor, Francis D. Nichol, whose study *The Midnight Cry: A Defense of William Miller and the Millerites* (1944) this book now replaces as a comprehensive history. Knight tells the story of Adventism after the "great disappointment" of October 22, 1844, and considers how Millerism shaped the rise of subsequent Adventist denominations.

In interpretation, too, Knight bridges the gap between new and old. One will find here a rejection of Nichol's apologetics, a frank consideration of how Seventh-day Adventism emerged from more radical Millerite reactions to the great disappointment (an issue that Nichol did not wish to consider), and the incorporation of all recent scholarly explanations of

the movement's short-term success. This synthesis is possible for Knight because scholars now accept part of what Nichol had previously asserted, that the Millerites were not all lunatics. Because scholars have interpreted the Millerites more objectively, Adventists now can look at their own history more objectively as well. (One might wish, in that case, that we could have had a title without the word "fever," a sign of pathology.)

In fact, the significance of this book is not so much what it tells us about Millerism yesterday but what it says about the Seventh-day Adventist church today. Millenarians are antihistorical; so Adventists were once interested only in histories that defended them from attacks by "the world," that is, apologetics. Knight, however, a Seventh-day Adventist, sees in Adventist history the story of faithful witness. Knight's thesis is that "Millerites were mission driven because they saw themselves as a prophetic people with a message that the world desperately needed to hear" (p. 10). Some historians will see in this continuing apologetics, but in reality it is devotional. Knight wrote this book for a general audience, but he also wrote it with his own church members in mind, to convey the idea, particularly in the last two chapters, that their history has meaning that informs their mission in the present. This devotionism is a mark of a mature denomination, secure in its faith, but it is a quality with which some academic historians will be uncomfortable. Still, Knight does not buy devotion with the price of historical accuracy or objectivity. It is not only a genuine history but also one that is comprehensive and useful.

DAVID L. ROWE
Middle Tennessee State University

PEGGY CHAMPLIN. *Raphael Pumpelly: Gentleman Geologist of the Gilded Age*. (History of American Science and Technology Series.) Tuscaloosa: University of Alabama Press. 1994. Pp. xiii, 273. \$49.95.

Raphael Pumpelly was not one of the most significant geologists in late-nineteenth-century America. He did not rival Clarence King or John Wesley Powell as a powerful administrator of a large, federal agency. He was not known for having made major discoveries or notable theoretical contributions to geology in the manner of a Grove Karl Gilbert or T. C. Chamberlin. Yet, as Peggy Champlin points out in this fine study, Pumpelly's interests and career embodied many of the important features that characterized not only geology but also the broader scientific culture of the time.

Pumpelly was a specialist, an economic geologist, and Champlin discusses his scientific training in Germany and his work in mining geology in Arizona, Michigan, China, and Japan. Pumpelly conducted a census of the mining industry for the U.S. Geological Survey, and later, when financier Henry Villard asked him to explore the area served by the Northern

Pacific Railroad, Pumpelly formed a Northern Transcontinental Survey that emphasized the study of economic resources. In Champlin's view Pumpelly practiced Humboldtian science: rigorous studies that included a concern for accurate measurements, the search for natural laws, and visible representations of data.

Yet Pumpelly was more than a specialist, and Champlin emphasizes the multifaceted character of his life and work. As a mining geologist Pumpelly combined specialized knowledge with a love of exploration. His researches in Asia gained attention as much for their novel, even romantic qualities as for their scientific contributions. An expert in the microscopic analysis of rocks, he also investigated the archaeology of Turkestan and the origin of Indo-European languages. He was not a teacher, but by conducting field work and directing research teams he profoundly influenced younger associates. Pumpelly, in short, represented the Victorian gentleman geologist: a well-educated, professional scientist who combined technical expertise with a love for the outdoors and an interest in a wide range of topics.

Champlin's study is strongest in bringing forth the complex qualities that characterize Pumpelly and nineteenth-century science. The author illustrates how his professional interests dovetailed with a love of art and nature on the one hand, and a distaste for academics on the other. She explains how Pumpelly could pursue his own scientific research interests while also fulfilling the practical objectives of his employers. Some topics should be explored in greater detail. The reasons for Pumpelly's early success are never made clear. Although Champlin permits Pumpelly's contemporaries to note his weaknesses, she never fully assesses his status as a scientist. Pumpelly was not the only generalist in an age of increasing disciplinary specialization; so too were James Dwight Dana and Joseph LeConte. Why Pumpelly is less well known than those or other figures deserves a fuller explanation. Given the current interest in questions of scientific visualization, an analysis of Pumpelly's maps would enhance the work's significance. Still, this study, by effectively portraying the subtle, nuanced complexities of one individual, offers important insights into the culture of late-nineteenth-century science.

RONALD RAINGER
Texas Tech University

DICKSON D. BRUCE, JR. *Archibald Grimké: Portrait of a Black Independent*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1993. Pp. xv, 269. \$24.95.

In 1890 a paleontologist argued in the journal *Open Court* on the basis of Lamarckian principles that "racial characteristics made 'American-Africans' wholly unassimilable into American society and its institutions." Archibald Grimké was ready with a

characteristically blunt and blistering response: it was ridiculous, he maintained, that the "'Indo-European race,' which had engaged in the worst exploitation imaginable, not only of blacks but of other 'races,' should be celebrated for its purity of character" (p. 65). It is hard for us to reconcile a Grimké vilified by his enemies as "a hireling of Tuskegee" (p. 140) with the Grimké whose uncompromising militance comes across in this book. His contemporaries found it equally difficult to classify him.

Newspaper editor, journalist, biographer, lawyer, political activist, and diplomat, Grimké was a consistent follower of neither W. E. B. Du Bois nor Booker T. Washington. Rather, Grimké accepted what he thought to be the most sensible views of each of these more visible and charismatic leaders, and rejected those positions that struck him as short-sighted or ill-advised. Sometimes he worked with Washington toward shared goals, sometimes he opposed him publicly, sometimes he challenged him through thinly veiled allusions in print. His relationship to Du Bois was equally complex. All that Grimké could be with any consistency was independent, a fact aptly underlined by the subtitle of Dickson D. Bruce, Jr.'s readable biography.

Bruce describes a quasi-free adolescent Grimké in Charleston who defies and eventually escapes from the half-brother who tried to reenslave him. Bruce examines Grimké's white abolitionist aunts from Boston, Sarah Grimké and Angelina Grimké, who mentored and nurtured his career out of a complex blend of family pride, family shame, and a personal sense of justice. Grimké's life was shaped by the courage and pain of his slave mother, Nancy Weston, as well as by the dedication and commitment of his famous aunts. Like Du Bois, he understood on a personal level that the color line was a fiction; he embraced the best of both of his heritages, black and white, as he made his way through one of the most racially charged periods in American history.

Best remembered for his articles on racial issues, Grimké wrote with insight on a range of other subjects as well, such as women's rights and women's suffrage (issues that had been important to his aunts) and the expulsion of the Jews from Russia, which he condemned as a "crime of international proportions," noting parallels "between the fate of the Jews in Russia and the vulnerability of blacks in the South" (p. 65).

Bruce might have allowed Grimké to speak for himself more often, rather than constantly paraphrasing him. He also might have devoted more time to Grimké's responses to female African-American intellectuals with whom he was acquainted, such as Anna Julia Cooper and Ida B. Wells. (One wonders what debt, if any, Grimké's outspoken articles on lynchings owed to pieces in a similar vein that Wells had published more than a decade earlier.) The book's later chapters are rather dry and certain events fall flat; David Levering Lewis's description of the "Bos-

ton Riot" in *W.E.B. Du Bois: Biography of a Race, 1868-1919* (1993) is infinitely more effective and engaging than Bruce's.

Overall, however, Bruce's book is well researched, well constructed, and useful to anyone interested in American history and American race relations during those turbulent years when Grimké was a major figure on the national scene.

SHELLEY FISHER FISHKIN
University of Texas,
Austin

DAVID M. WROBEL. *The End of American Exceptionalism: Frontier Anxiety from the Old West to the New Deal*. Lawrence: University Press of Kansas. 1993. Pp. x, 233. \$27.50.

In 1914, worried about the disappearance of the American frontier, the journalist Ray Stannard Baker hoped that a new pioneering spirit would appear. Although the frontier had rallied people to the cry of "Westward Ho!" this new spirit, Baker thought, would have to respond to the call "Inward Ho!" or even "Downward Ho!" (p. 94).

These memorable cries, to David M. Wrobel, are symptoms of anxiety. Examining a wide range of books, as well as articles in newspapers, magazines, and journals, Wrobel tracks the symptoms of "frontier anxiety" in many late-nineteenth and early twentieth-century intellectuals.

Where did frontier anxiety come from? Europe, the theory had gone, was a mess, but the United States was not. The explanation for this difference was as simple as the situation it explained: the United States had a frontier, but Europe did not. The abundance of land open to ownership by white Americans made the United States unique, exceptional, and quarantined from the social tensions and struggles of Europe.

And yet, if the frontier was the fuel for American well-being, what would happen when that fuel ran out? This was a plot that permitted only one ending: with the exhaustion of free land would come the end of American exceptionalism, and a growing similarity between European and American dilemmas.

This anxiety became noticeable in the 1870s, more than a decade before the director of the Census and Frederick Jackson Turner officially announced the end of the frontier. Wrobel tracks this concern through to the 1930s, noting its impact on debates about conservation, immigration, overseas expansion, and democracy's prospects. It is particularly instructive to see the impact of frontier anxiety on the New Deal, recast this time as a conviction that government must now perform the social functions once fulfilled by the frontier.

Wrobel's point, he observes repeatedly, is that "numerous Americans perceived that the frontier had closed, and acted on their perceptions." It is, by contrast, not at all his intention to ascertain "when or whether the frontier closed" (p. viii).

These repeated assertions of neutrality on the disputed issues of western American historiography suggest that frontier anxiety might not be the only anxiety at issue here. Western American history, Wrobel recognizes, is a field in which opposing schools clash: Old Western historians versus New (or Middle-aged) Western Historians, frontier historians versus regional historians. By one theory, such a field is embattled territory where keeping one's arguments and assertions close to the ground seems the best strategy.

But by another theory, such a field presents territory where a new arrival's fresh and forceful interpretation stands a chance of setting everyone free of debates and disagreements that have become routinized and stale. With his thoroughness of research and his instinct for selecting provocative and telling examples and quotations, Wrobel's future work may well provide just such a service. But if the climate of scholarly inquiry convinces young scholars of the accuracy and wisdom of academic anxiety, then western American historians may join Ray Stannard Baker in the cry, "Downward Ho!"

PATRICIA NELSON LIMERICK
University of Colorado,
Boulder

DAVID MONTGOMERY. *Citizen Worker: The Experience of Workers in the United States with Democracy and the Free Market during the Nineteenth Century*. New York: Cambridge University Press. 1993. Pp. x, 189. \$21.95.

Readers familiar with David Montgomery's earlier books, or with the work of some of his students, will recognize many of the major themes in this new effort. Originally given as the Tanner Lectures at Oxford University in 1991, these now revised talks address the condition of American working people during the nineteenth century. Montgomery makes three broad points: democracy and free-market capitalism were inimicable; in order to "police the people for the free market" (p. 52), the apparatus of the state expanded the power of the bourgeoisie while limiting the ability of workers to make effective demands on government; and that the major political parties neither "were nor could become a workers' party" (p. 115). Girding this three-pronged discussion is the fundamental assertion that over the century "government's coercive power contributed to the hegemony of business and professional men . . . [but it] was the working people who sought to preserve the community welfare" (p. 12) in both public and private spheres.

Along the way Montgomery stresses the importance of restrictions on democratic rights such as indentures and slavery, although he must acknowledge the rather high percentage of electoral participation among white men in the post-Civil War decades. He emphasizes the manner in which elites and middle-class reformers utilized an evangelical spirit to control

working people's "customs and codes" in an era of industrial change, although he never recognizes that workers, too, attended church and shared in complex spiritual rituals. Montgomery also argues that a unified causality explains such events as the professionalization of the police, the privatization of poor relief, the postwar Black Codes (especially concerning vagrancy), reform of military service, and the public regulation of housing: as he explains in reference to the last point, the "taproot of that disorder was the market system itself" (p. 104). Finally, despite the valiant efforts by various "labor activists," the self-conscious presence of the state as a representative of elite business interests prevented the emergence of independent, working-class political parties and subverted the essential meaning of democracy. Military force, Montgomery writes, was the "ultimate bulwark of a free-market economy" (p. 104).

Over against these obstacles to virtue, Montgomery invokes a cast of historical "characters" familiar from his earlier work. "Codes of mutualism" and "solidaristic intentions" permeate workers in his constellation, and their actions frequently comprise "the stuff of group rebellion" (pp. 55–56)—this in a manufacturing community in 1797. "Labor activists" often stand for the group as a whole, and isolated actions of other individuals are pressed into service on behalf of a history too burdened with theory. The suggestion, for example, that Union General August Willich was "but the most prominent of the officers who regularly educated their troops with a Marxist interpretation of the war in which they were engaged" (p. 97) reveals, in its singular disregard for the voluminous literature on Civil War-era political culture, just how far Montgomery is willing to stretch to "prove" a consistent theory.

It is not puzzling that Montgomery focuses primarily on the relations of production or that he sees the nineteenth century as an era of lost liberty. Rather, the fundamental problem is that the author does not engage in a sustained fashion the ideas of others. He relies most heavily on those who share his perspective, and where he does cite others, he uses them as a selective data base and does not consider their ideas. The result is a skewed book. General Willich and "Count" Joseph Rybakowski, the leader of the "Industrial Army [that in 1894] enjoyed the overwhelming support of Buffalo's Polish community during its violent clashes with the city police" (p. 156), among others, play cameo roles in this analysis insofar as they might imply an evidentiary basis for the interpretive scaffolding. The inference never takes, and the lack of broader intellectual engagement with those who might disagree reduces these essays to a series of historical polemics, parts of which are of more interest than others.

NICK SALVATORE
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KIM VOSS. *The Making of American Exceptionalism: The Knights of Labor and Class Formation in the Nineteenth Century*. Ithaca: Cornell University Press. 1993. Pp. xv, 290. Cloth \$39.95, paper \$16.95.

Kim Voss's argument has four parts. First, the trajectories of the labor movements in the United States, Britain, and France were similar from the 1830s until the 1890s. Second, after 1890 the American labor movement remained weak, conservative, skilled, and essentially apolitical, while the British and French labor movements became stronger, more radical, organizers of the unskilled, and politically mobilized. Third, the turning point for the United States was the demise of the Knights of Labor, a radically reformist organization of all workers, regardless of skill, race, ethnicity, or gender. The Knights' collapse established a legacy that advocates of radical class politics and inclusive organization were never able to overcome. Fourth, previous explanations for the Knights' failure are not confirmed by an empirical study of the Knights of Labor in a representative state (New Jersey). Voss disputes scholars who attribute the Knights' demise to a middle-class outlook and those who stress industrial and ethnic diversity. By contrast, she emphasizes employer opposition.

The author has landed herself in the midst of the classic question of American labor history: why American workers were "different." As Voss notes, some historians have expressed impatience with unending reworking of this issue. Voss devotes herself to demonstrating its relevance with verve and rhetorical skill.

The first three parts of her argument are based on her deft analysis of other scholars' works. Few specialists will find any surprises, but, as she notes, these specialists usually have not explicitly pointed to the parallels she sees. Her case for these parallels strikes me as reasonable but insufficient, a reflection of weaknesses in the literature on which she draws. For example, labor historians in all three countries have emphasized radical, male artisans, and that is where Voss finds her strongest parallels. Scholars have written less about other types of workers, and so does she. She does not discuss the impact of slavery, race, or region for American working-class development. Neither does she really unpack the concept of exceptionalism; if Britain and France diverged from the United States, why does that make the United States "exceptional"? Exceptional compared to what norm?

Her case study of the New Jersey Knights is also thought-provoking but sometimes constrained by her sources and methods. She evaluates hypotheses for the Knights' demise by creating a data base for all of New Jersey's Knights of Labor assemblies and trade unions and the characteristics of the communities in which they were located. She then uses sophisticated statistical models to test the impact of community variables on the longevity of Knights of Labor locals and the success of craft versus industrial organizing strategies. Using this analysis, she argues that conflict

between skilled and unskilled workers was statistically relatively unimportant. Indeed, in some situations, the presence of strong craft organizations facilitated rather than discouraged organization of the unskilled. She also finds that the degree of ethnic and industrial diversity in a community does not statistically predict the probability of survival of Knights of Labor locals.

If these statistical results accurately reflect what happened, they are important discoveries, but some findings may be a function of how she coded and organized her data. Does the statistical relationship between an index of ethnic dissimilarity in a community and the persistence of Knights of Labor assemblies in that community adequately capture the role of ethnic conflict? In a qualitative account of the Knights in Newark, the author finds more important ethnic cleavages than the statistical results would suggest. She is sensitive to these potential problems with her methods and takes pains to supplement her statistical analysis with literary sources.

The questions I have posed about sources and methods thus do not detract from my estimate of the value of this work. I found it to be clearly written and well argued. Even where she did not completely convince me, Voss forced me to rethink my own arguments. Future scholars of American working-class development will not be able to ignore this book.

RICHARD OESTREICHER
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MELVYN DUBOFSKY. *The State and Labor in Modern America*. Chapel Hill: University of North Carolina Press. 1994. Pp. xviii, 321. Cloth \$34.95, paper \$14.95.

In recent years labor unions have all but disappeared from American historical writing. They typically command no more than a paragraph or two in introductory texts and rarely figure prominently in specialist studies in economic or social history. This trend reflects several influences, including the lowly state of organized labor in contemporary society. In his new work, which employs the word "labor" in the older sense of organized labor or the labor movement, Melvyn Dubofsky vigorously rejects the modern tendency, reminding readers that organized labor was at the heart of public-policy debates for many decades. Historians, he argues, ought to take history seriously and not simply read backward from today's newspaper headlines.

There is no shortage of material to support this perspective. Dubofsky's principal contribution is to synthesize a vast amount of specialist, often fugitive writing on the relations between government and unions from the 1870s to the 1950s. He examines in detail the link between Progressive-era activism and organized labor, the fragile but profitable alliance between the American Federation of Labor and the Woodrow Wilson administration, the ambivalent status of organized labor in the 1920s, the labor law

revolution of the 1930s, the growth of labor power in the 1940s, and the passage of the Taft-Hartley and Landrum-Griffin labor acts. Dubofsky devotes slightly more than 10 percent of his work to the nineteenth century and a similar proportion to the last half century. The two chapters on the 1930s, approximately 40 percent of the book, are the core. Combining his own notable research on the Industrial Workers of the World, the United Mine Workers, and the Progressive era with new work in government materials and a thorough survey of secondary works, he explains not only what happened but also why the fate of organized labor became a litmus test of social health for early twentieth-century reformers. This is the best available introduction to the labor movement in its larger, public context and to an important chapter in recent political history.

Like most scholars, Dubofsky has more difficulty explaining why the position of organized labor changed so quickly after the 1940s and why labor legislation has become more sweeping and intrusive while unions have declined in membership and popularity. A comprehensive explanation, if possible, would require a different and much longer volume. Dubofsky's account therefore is not the place to seek an understanding of recent labor legislation or the plight of the labor movement. It is, however, an expert guide to the half-century of political and legal turmoil that preceded Taft-Hartley and a persuasive reaffirmation of the significance of organized labor in twentieth-century history.

DANIEL NELSON
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MATTHEW W. ROTH. *Platt Brothers and Company: Small Business in American Manufacturing*. Hanover, N.H.: University Press of New England, for the University of Connecticut. 1994. Pp. x, 256. \$40.00.

Business history used to be relatively simple: Alfred D. Chandler, Jr., had provided a modernization framework for understanding the development of American business in the nineteenth and early twentieth centuries. It went something like this: the natural order of things was for businesses to progress from a simple, traditional form to a more highly complex one. "Modern" businesses were defined as multi-unit, hierarchically managed firms selling standardized, mass-produced items to a national and international market. Those firms that did not fit this archetype were, by definition, not modern, and relegated to the dustbin of history. In American history textbooks, students soon learned that massive Lowell textile mills in the antebellum period were the norm, and that large railway and steel mill enterprises were typical in the late nineteenth century. These were then succeeded by massive automobile and other producers for the mass consumer market.

This archetype continued relatively unchallenged until the appearance of the work of Philip Scranton in

the early 1980s. He focused on a different kind of firm, one that he argued was far more typical of America during these years. Scranton's firms were smaller, single-unit producers that engaged in batch production or specialized products for niche markets. Matthew W. Roth builds impressively on Scranton's work.

Platt Brothers and Company was a small, family owned business that engaged in the nonferrous metal trade in western Connecticut. Roth's research and writing have been, as is often the case, subsidized by the firm. Roth claims that the company placed no restrictions on his writing, and I detected no bias. With seemingly full access to corporate records, particularly through the 1920s, Roth has written a competent, detailed history of a firm that is probably much closer to the American norm during those years than the megabusineses analyzed by Chandler. Roth follows an essentially chronological framework, detailing the business decisions made by Alfred Platt, the family founder, his sons, and the later, non-family executives who assumed management of the company. Even when under the control of others, however, Platt remained essentially a family firm until the 1960s, following traditions of paternalistic labor relations, plowing substantial portions of profits back into plant improvements, and pursuing new niche markets with an aggressive conservatism.

Platt Brothers products were not among those that have fired the imagination of earlier historians. They made buttons and other small products from zinc, mostly in small batches, to a group of steady customers. In the twentieth century their most important product was the small strips of zinc that were used in fuses. The firm was sold to a conglomerate in the 1960s, after which the family management techniques ended.

Roth has written an important book, but one that will need to be matched with numerous studies of businesses in other industries in different parts of the country in order to be able to separate the unique from the typical. It does, however, represent an important step in the path toward a new synthetic understanding of American industry.

JOHN N. INGHAM
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CHRISTOPHER WALDREP. *Night Riders: Defending Community in the Black Patch, 1890–1915*. Durham, N.C.: Duke University Press. 1993. Pp. xii, 264. Cloth \$45.00, paper \$16.95.

In this study of an agricultural region of Kentucky and Tennessee called the Black Patch (after the dark variety of tobacco grown there) Christopher Waldrep tells how monopolistic organizations such as James B. Duke's American Tobacco Company and the Italian "regie" (the farmers' term for any foreign regime), a cartel of foreign interests that purchased 80 percent of Black Patch tobacco, expanded into rural America.

Tobacco farmers responded with the Planters' Protective Association (PPA) to defend against the efforts of big business to control the price of local tobacco. To co-opt independent-minded small farmers, the planter-led PPA resorted to Night Riders, a secret (not even their families knew in many cases), oath-bound organization of men led by a doctor's son that stormed through the night on horses, terrorizing those who sold to the tobacco trusts. Neither race nor economics distinguished vigilante from victim; riders came from a variety of backgrounds and at least one was a black man.

The first wave of violence in the Black Patch War victimized few blacks and targeted property—warehouses and tobacco barns where the companies stored their harvest—more than people. But by the end of 1908, a second wave of violence veered off the original track, as white iron workers—"shirt-tail night riders" (p. 140)—attacked the black tenants and sharecroppers whom planters needed as labor. As the violence broadened, became more chaotic, and threatened established authority, Black Patchers turned against vigilantism.

Besides challenging the body of scholarship that portrays rural southerners as determinedly resistant to modernization, Waldrep also strips away romantic notions about the independence of thought and action in nineteenth-century rural community structures. "The first characteristic [of them] was uniformity of thought" (p. 143). It may come as a surprise to many modern-day Black Patch residents to find their nineteenth-century forebears as Baptist deacons, judges, doctors, lawyers, and other pillars of the community committing extortion and manipulating the lower orders to promote a homogenous society. But there is another, more sanguine Black Patch past: after the Black Patch War, when lawmen began standing up to lynch mobs, when judges and juries began to convict the lawless and unmask the vigilantes, when official toleration for vigilantism ceased, the mob violence ended. In other words, community leaders are ultimately responsible for the subculture of violence, whether in the form of the KKK, Night Riders, or ghetto gangs.

Waldrep's complex and nuanced study is the best account of the Black Patch War since historians and novelists began to write about that episode in the 1930s. Only in the case of black labor is the story blurred and unconvincing, seeing blacks as victims, more rarely as agents. Waldrep acknowledges researching and writing the book for fourteen years. It shows. The study began as a series of journal articles; parts of all chapters except two have appeared elsewhere. His imaginative use of a variety of sources, including manuscripts, newspapers, government documents, oral histories, unpublished court cases, and church minute books gives his account the voice of authority and the ring of authenticity.

CRANDALL SHIFFLETT

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JAMES H. LINDGREN. *Preserving the Old Dominion: Historic Preservation and Virginia Traditionalism*. Charlottesville: University Press of Virginia. 1993. Pp. xiii, 316. \$29.95.

James H. Lindgren has provided scholars a complex and well-researched study of motivations and strategies surrounding the development of the Association for the Preservation of Virginia Antiquities (APVA) from 1888 to 1939. Brought into existence by Mary Jeffery Galt and Cynthia Coleman in 1888 and granted a state charter in 1889, the APVA began when individual preservation efforts coalesced and adapted the earlier creation of the Mount Vernon Ladies Association of the Union. The founding members of the APVA achieved a consensus on a narrowly focused agenda to maintain the social hierarchy of Tidewater Virginia and to manipulate public opinion to support this goal through the use of object lessons from the past. A secondary goal of asserting the claims of primacy to Virginia colonization by the English against the dominant view of Puritan establishment of the American character, essentially a sectional dispute, was an important subtext of their early efforts.

Reacting to the twin calamities of the Civil War and the rise of industrial capitalism, the social elites of Virginia found themselves increasingly marginal to political power across the state. Although Reconstruction had ended in the 1870s, radical politics continued, especially in the administration of Governor William O. Mahone, whose readjusters sought to change the political and economic climate in Virginia. This social class especially hated the enfranchisement of African Americans under the Underwood Constitution of 1869. Antebellum social elites struggled to regain control of government through political actions and (the focus of this book) use of cultural symbols and selective cultural memory. The key political fulcrum within this time frame is the Virginia Constitution of 1902 and the large-scale disenfranchisement of African Americans. More subversive were the attractive and repulsive tendencies of industrial development. Progressive political leaders in Virginia recognized the need for new mechanisms of capital development within the state, but they worried about how the new wealth would be controlled and what new social alignments would be created by the new economic orientations. The elites' response was to encourage new industrial potential, but also to emphasize the personal qualities of hard work, honesty, self-sacrifice, family values, and philanthropy without calling for fundamental changes in the way government functioned. To ensure continued control, social elites stressed the values of states' rights.

More central to this argument Lindgren suggests that voluntary associations, especially the APVA, reinvented the past to provide moralistic lessons appropriate to the late nineteenth century. Lindgren uses

Anthony F. C. Wallace's model of a revitalization movement as a response to outside pressure and internal stress with its "selective adoption and discriminating application of those traditions" (p. 54). Preservation—that is, the stabilization of physical artifacts—was part of a larger conservative action that Lindgren describes as cultural traditionalism. The locus of traditionalism was a series of oppositions that appeared both as causes for action and evaluations of events. Traditionalism opposed social pluralism, venerated social position by birth other than achievement, sought to emphasize individual responsibility over environmental determinism, and stressed historical idealism over historical realism. Crucial to traditionalism was the importance of the object lesson. Selectively chosen historic figures and events became models of right actions and deeds. The object lesson allowed selectivity of inclusion, and the moralistic tone held implicit values that were unassailable. As James Alston Cabell remarked, "the virtue of one generation was transfused by the magic of example into several" (p. 54). The models were not developed to convince African Americans of the rightness of their low station; the models were targeting Virginia's new elite: men and women of business and the professions who adopted the manners of traditionalism. The ostensible early models resuscitated the images of colonial patriots, but this was always ambiguously interleaved with the memorializing of the heroes of the Civil War and the hagiography surrounding Robert E. Lee and his lieutenants.

Lindgren reintegrates the social and political agendas of conservatism by demonstrating that cultural traditionalism was hegemonic. The assumptions and the values expressed by traditionalism were accepted as true by Virginia's leaders and the white voters. Without denying lynchings and violence, he contrasts Virginia's experience with "those southern states which lacked Virginia's well-developed tradition of elite rule and social deference . . . [which] turned earlier and more conspicuously to physical intimidation and legal statute to restore order" (p. 243).

The difficulty was not persuading the newly powerful middle class of the virtues of traditionalism, but convincing this elite that material artifacts could symbolize what was important about the Old Dominion. "Many traditionalists were willing to let the buildings fall and to restrict their traditions to social discourse, formal education, or political conservatism" (p. 243). What did preservation give traditionalism? Lindgren suggests that preservationists transformed target buildings into tangible symbols of reified values that supported social stability and were evidence of the permanence of the Old Dominion. The process involved selective reduction of past events (often fictive), traditionalizing celebrations, placing commemorative plaques to solidify the intended interpretation, as well as laborious stabilizing and beautifying of the landscape and architecture.

The process was so demanding and selective that fewer than twenty properties had been purchased by the APVA by 1939.

The APVA began as a women's preservation organization whose spokespersons were often, but not exclusively, men. In contrast to the preservation organizations of New England headed by men, the APVA tended to blend the intentions of preserving architecture with the civil religion of patriotism, love of family, and filiopietism. Although much is made of the seminal role of the APVA as the progenitor of modern preservation, the forbears of responsible usufruct in the late twentieth century as practiced by the National Park Service, Colonial Williamsburg, and even the APVA are more a product of New England preservation efforts, discussed in Lindgren's companion book, *Preserving Historic New England*. The APVA was, during this time period, an amateur organization with little skill in assessing the integrity, the dating, or even the significance of artifacts. The central example of the organization's true impact was the purchase and exhibition of Jamestown Island. As Lindgren notes, "nothing better illustrates the APVA's work than the transformation of this barren island into a powerful symbol" (p. 2). Strangely it is not the preservation of buildings or the assertion of a state-wide agenda, but the legacy of commemorative and symbolizing activities of the APVA that are important continuities with modern preservation. Virginia would take up the commemoration of historic activities in 1926 with the roadside marker program that exists today. And the APVA would itself abandon the model of museum sites in order to further the preservation of historic buildings, choosing to have many properties productively used in the present, rather than mothballed for visitors.

Lindgren uses a rich metaphoric language that at times reads as Victorian-inspired as the subject. For example, his introduction of traditionalism ("Virginia was much like a burned forest, however. Its rich ashes eventually would yield to the strongest physical growth and that was the revival of traditionalism" [p. 13]) creates a metaphor that explains too much, or nothing at all. His writing style is brisk and enjoyable, however, and his approach is complex and insightful and deepens our understanding of the motives of people already introduced through Charles B. Hosmer, Jr.'s *Presence of the Past: A History of the Preservation Movement in the United States before Williamsburg* (1965). This is an organizational history that focuses on the social ambitions of (well-connected) social elites, but Lindgren demonstrates that early preservationists sought national recognition to force "an acceptance of the propriety, validity, and effectiveness of the Old South civilization. The past became a prologue to the present" (p. 245). By extension, when modern preservation accepts APVA as precursor it accepts its interpretation. When people wonder at the occa-

sional antipathy of African Americans to the preservation movement they should consider the source.

GARY STANTON

Mary Washington College

ELISABETH LASCH-QUINN. *Black Neighbors: Race and the Limits of Reform in the American Settlement House Movement, 1890-1945*. Chapel Hill: University of North Carolina Press. 1993. Pp. xii, 225. Cloth \$39.95, paper \$14.95.

Traditionally, the history of the American settlement-house movement told the story of the settlements affiliated in the National Federation of Settlements (NFS), founded in 1911. These agencies were non-denominational because the NFS excluded religious settlements (missions) from membership. Located mainly in immigrant neighborhoods, the NFS settlements had few contacts with blacks, although some settlements established separate black branches. Settlement-house historiography detailed the campaigns waged by the most activist settlements to expose the evils of urban-industrial society; it was about Progressive reform, about immigrants and Americanization, and about white women's activism. Settlement historiography did not intersect with the narratives of black history. It excluded agencies founded by and for blacks, the South, and missions (these categories often overlapped), even though religiously inspired and denominationally funded agencies combining service and reform outnumbered NFS-member settlements.

Social-work historian Judith Ann Trolander was among a number of scholars who offered a new perspective on the settlements. In her book *Professionalism and Social Change* (1987), she looked at settlements that served blacks and extended the "movement" beyond the Progressive era, surveying an entire century of settlement activity. Elisabeth Lasch-Quinn continues this revisionist trend. She rejects the narrow definition of a settlement as a member of the NFS and considers a variety of agencies organized for and by African Americans. Her criteria are broad: she includes any organization that "conducted extensive work in black neighborhoods in the settlement house tradition" (p. 115). She recounts the activities of Methodist home missions and YWCAs, "school settlements," and "Bethlehem Houses," across the Midwest, Northeast, and the South.

Lasch-Quinn is most effective where she discusses particular settlements, such as Karamu Settlement in Ohio and the Wharton and Belmont centers in Philadelphia. Her assessment of these and other black settlement-type agencies is positive. For example, in discussing Calhoun Colored School in Alabama, a school that functioned "as a 'community center,'" (p. 89), Lasch-Quinn takes issue with critics of industrial training programs for blacks such as John H. Stanfield, Donald Spivey, and Elizabeth Jacoway. She describes Calhoun's curriculum as mainly academic

rather than vocational and asserts that the organization's efforts to help blacks purchase land and become independent farmers amounted to "a serious attempt to alter social conditions" (p. 96).

Yet this study is unconvincing at several points. Lasch-Quinn is burdened by difficulties of definition. (What kind of social-service agency would not be defined as a settlement?) And because the author accepts the settlements' claims that they correctly interpreted the needs of clients, she is generally uncritical of the influence of donors and trustees on settlement programs. Lasch-Quinn devotes little attention to tensions between service providers, donors, and clients.

Most disturbing is Lasch-Quinn's cavalier dismissal of recent scholarship on the settlements. Her statement that "Scholars have accepted uncritically the self-image developed by the settlement movement" (pp. 5-6) implies that historians have not previously considered the role of race or religion in the settlement movement. This misrepresents or short-changes the contributions of a number of scholars of the black settlements, including Howard Jacob Karger (*The Sentinels of Order* [1987]), Ralph E. Luker (*The Social Gospel in Black and White* [1992]), and myself (*Social Work and Social Order* [1992]). Her discussion of the "mainstream" settlement movement fails to incorporate Rivka Shpak Lissak's important book debunking Hull House pluralism (*Pluralism and Progressives* [1989]) or Mina Carson's sensitive reassessment (*Settlement Folk* [1990]), relying instead on the older view espoused by Allen Davis, Clarke Chambers, and John Higham of the settlements as liberal and progressive. (In his "introduction" to the 1984 edition of *Spearheads for Reform*, Davis acknowledged the need to explore coercive and racist aspects of the movement.) Her "finding" that the NFS barred religious missions even though many were identical to settlements essentially reiterates the discussion in my dissertation (1982). Her statement that "former settlement studies end in World War I" (p. 6) is inaccurate.

Where Lasch-Quinn does offer a new interpretation of the settlements is in the final section, "From Mother Power to Civil Rights." She is right to insist that we must broaden the definition of Progressivism to include the valiant reform work of black women. But her hypothesis that black and white women working together in settlement-type organizations (she includes the YWCA) laid the foundations for the civil rights movement is asserted, not demonstrated. Thus, the story of how early twentieth-century women's reform became the civil rights activism of the 1960s is still to be written. Finally, Lasch-Quinn's assumption throughout her study that the settlement movement is now defunct should not go unchallenged: at latest count, thirty-eight settlements were still functioning in New York City alone.

RUTH CROCKER
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ALICE SHEPPARD. *Cartooning for Suffrage*. Foreword by ELISABETH ISRAELS PERRY. Albuquerque: University of New Mexico Press. 1994. Pp. xxviii, 276. \$37.50.

In preparing her welcome new work, Alice Sheppard combed the archives to recover a substantial number of political cartoons published in the first decades of the twentieth century in support of the woman's rights movement. Most of the cartoons were originally published in suffrage journals by women artists whose names and work are largely unknown to us today. In introducing us to this genre, Sheppard offers more than 200 examples of the cartoons, accompanied by an extended examination of the themes, images, and metaphors represented in the cartoons. She also establishes the cartoons within the context of the suffrage movement, the history of political cartoons, and modern feminism. This is an ambitious, successful scholarly work that furthers our understanding of the complexities of the woman's rights movement, dispelling the image of the suffragists as humorless zealots.

I expected a book primarily of cartoons, with some explanatory text, but instead found that essentially equal space is devoted to text and illustrations. This is at once the strength and weakness of the study. Sheppard has done far more than edit a book of suffrage cartoons: she offers three lavishly illustrated chapters of historical background, one chapter on the lives of the cartoonists, and four chapters that examine individual cartoons in terms of their themes and imagery. Sheppard rounds out the book with a discussion on suffrage art and feminism. Written text occupies half of each page, while the outside margins are reserved for the illustrations, usually two per page. As a result, some of the cartoons are too small to be fully legible. In this case, the press made a production decision that detracts from the central interest of the book. (It is also unclear why the press felt the need for a separate introduction, by Elisabeth Israels Perry, who seems more interested in critiquing nineteenth-century feminism than enhancing our appreciation for the accomplishments of the cartoonists or the scholarship theoretically being introduced.) Nonetheless, this is a handsome book whose illustrations and accompanying textual analysis are sure to be of continuing interest to teachers and scholars.

In one of her most interesting single chapters ("Becoming a Suffrage Cartoonist"), Sheppard identifies eleven different suffrage artists whose work features prominently throughout the book. Almost all of these women were born in the 1870s, most came from old, established American families, and many had received formal training in art at "prestigious art schools and colleges" (p. 104). In the absence of extensive biographical information about a number of the artists, Sheppard weaves instead an intriguing composite picture that gives the reader a firm sense of the independence and drive of these women. Nonetheless, a few distinct personalities shine through,

which causes me to hope that Sheppard will revisit some of this terrain to write an extended biographical sketch of one of the more colorful of the artists, such as Nina Allender, or the most prolific cartoonist of the era, Lou Rogers.

Rogers was born and raised in Maine, used her own earnings to attend the Massachusetts Normal Art School in Boston, then moved to New York's Greenwich Village and became a member of the radical feminist club, Heterodoxy. According to Sheppard, Rogers was "the earliest American woman to produce a series of suffrage cartoons," ultimately publishing her work in *Judge*, *The Woman's Journal*, *The New York Call*, and *The Woman Voter* (p. 106). Her cartoons, which appear throughout the book, are bold, outspoken, and visually compelling, such as her "Tail of the Comet" in which a comet named Woman Suffrage (represented by the head of a woman) streaks across the sky, followed by a wide swath of comet tail labeled "A Higher Rate of Intelligence for all Humanity" (p. 131). My own favorite, because of its imagery and sense of humor, depicts a female farmer trying unsuccessfully to drive her mischievous, scattering pigs (labeled "Reform Measure") toward a barn identified as "Effective Legislation Market"; she has nothing to aid her but a club labeled "Moral Suasion." The next frame shows a row of startled pigs sliding orderly down a chute labeled "The Vote," with the farm woman standing on the top of "The Home," and hoisting the last pig by its ears; she is firmly in control (p. 172).

I like the ambition of this book and the scholarship it might engender, such as a study devoted exclusively to the humor of the cartoons, or a more extended analysis of the way the leaders of the suffrage movement countered their opposition. The fact that it leaves us wanting to know more about the cartoonists and their work only attests to the inherent interest in the topic as enhanced by Sheppard's careful scholarship and clear prose style. This volume promises to be an invaluable resource for anyone interested in the history of the woman's rights movement, women's political and social history, or women's humor.

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GENEVIEVE G. MCBRIDE. *On Wisconsin Women: Working for Their Rights from Settlement to Suffrage*. (History of American Thought and Culture.) Madison: University of Wisconsin Press. 1993. Pp. xxiii, 352. Cloth \$43.00, paper \$19.95.

Genevieve G. McBride's study is informative and useful for the student of suffrage history. Local women's rights histories are scarce, and this book helps fill the gap. One of McBride's most interesting finds is that although Wisconsin has been known as one of the Progressive states in its legislative reforms of the

early twentieth century, its populace resisted woman's suffrage long and hard.

McBride's focus is not just woman suffrage in Wisconsin but also other areas of women's involvement in public affairs, notably in the late-nineteenth-century temperance struggle and in the explosion of women's club civic activism. She stresses the importance of good "press agency" for each of the movements "as a means of communicating strategies and tactics of their crusade" (p. 60). One strength of this study is the featured individuals: Wisconsin boasts some major reformers, including Frances Willard and Carrie Chapman Catt, but we also are introduced to the hard-working temperance journalist Emma Brown and to Theodora Winton Youmans, newspaperwoman/suffragist extraordinaire. Both women figure prominently in Wisconsin women's activism, according to McBride, because of their crucially important "press agency" talents. One fascinating aspect of the book is the account of the racism that led to the pointed exclusion of the nationally prominent black clubwoman, Josephine St. Pierre Ruffin, from a General Federation of Women's Clubs meeting in Milwaukee, duly recorded by journalist Youmans, who relished "the fracas" (p. 176).

For all its usefulness, the book does have problems. It is not always the most readable book, nor is it usually analytical enough. McBride has accounts of both the press reaction to Wisconsin women's activism and women's attempts to control publicity, but there is no overall thesis made about the press' role. At one point she discusses Willard's "Home Protection" slogan as an important moment in "mass persuasion history" (p. 109), an interesting concept from which an overarching analysis might develop, but it does not. The reader waits in vain for an analysis of the grass-roots membership of any of the groups discussed. Women's history contexts are not always spelled out or historiographically up to date. The author seems overly negative toward certain figures, especially the Reverend Olympia Brown, who, McBride says, used "centralization to the extreme" (p. 105) in her management style and was "out of touch" with Wisconsin women (p. 198). McBride is also very critical (and in this she is hardly alone) of the "strident" Alice Paul's National Woman's Party with their suffrage "stunts" and organizing drive "machinations" in Wisconsin (pp. 269, 282). Sometimes the author is somewhat contradictory and arbitrary in assessing successful "press agency," and in both praising and condemning the split in suffrage groups. She remains interested only in differences in "tactics" and not in feminist philosophies, whether discussing temperance, women's club issues, or woman suffrage.

Despite these weaknesses, the book does make a contribution in the area of local histories of women's reform efforts. It will be especially welcomed by those

interested in local women's fights for temperance, suffrage, and their rights.

LINDA G. FORD
Keene State College

JEFFREY A. CHARLES. *Service Clubs in American Society: Rotary, Kiwanis, and Lions*. Urbana: University of Illinois Press. 1993. Pp. x, 226. \$32.50.

Accounts of American service clubs have divided between critics and defenders. Unlike these works, Jeffrey A. Charles's volume offers a close, well-contextualized, and even-handed if sympathetic account of the clubs. His focus runs from 1905 to 1917, when Rotary, Kiwanis, and Lions were founded, to 1940. Attention is given to nineteenth-century predecessors of the clubs, particularly fraternal organizations. Later, Charles tracks the clubs' migration from small towns to suburbia.

The picture that unfolds is familiar. The clubs were populated by small businessmen and locally oriented professionals. Initially they attempted to limit membership to one person from each trade so that clubmen would prefer each other in business dealings. Uneasy at criticism, the clubs removed the restriction and muted talk about the economic advantages, turning instead to community service. "He profits most who serves best" became Rotary's slogan.

This meant no diminishment of boosterism, or of camaraderie. At their weekly meetings the clubs cultivated the boyish informality and delight in hijinks that Sinclair Lewis and H. L. Mencken satirized. These habits, which diminished over the decades, represented more than a bias of style; at international club conventions, Americans insisted that "sociability" was the key to world peace.

Behind this language, behind lofty affirmations of community service and child-like effusions of camaraderie, Charles's book finds its thesis. At its most succinct, as in footnotes to the first couple of chapters, the argument of the book is located among works that emphasize the importance of the emergence of bureaucratic, centralized America. It is not the institutional side of these accounts that interests Charles but the "less rational reactions and motivations and the tensions and ambivalence that invariably accompany the inexorable march of bureaucratic structures" (p. 161).

His book mirrors recent works on nineteenth-century voluntary organizations, stressing the ambivalent response of America to the growth of a corporate order. Essentially business organizations, the clubs nonetheless had mixed responses to the rise of big business. They envied the scale of its financial success, but feared for the continued profitability of their local or regional enterprises. This never drove them into a small business opposition to big business, nor to identification with the old middle classes rather than the new.

They were, Charles says, transitional organizations

between old values and the new. What is revealing about them is not their community service activities—few of these were "systematic or sustained" (p. 76)—but the expressive, not instrumental, values displayed in the fellowship and social solidarity that marked their weekly meetings.

How to estimate this assertion goes to the question of how to estimate popular culture. There has been a strong recent preference in the profession, reflected in Charles's citations, to invoke psycho-social explanations for this purpose. How illuminating this may be is open to question: spokesmen for labor, whether unionized or not, found their interpretations of the service clubs in other quarters. Perhaps they were wrong.

JOHN TOMSICH
Reed College

RICHARD A. EPSTEIN. *Bargaining with the State*. Princeton: Princeton University Press. 1993. Pp. xvi, 322. \$35.00.

In this book, Richard A. Epstein returns to some themes introduced in his earlier book (*Takings: Private Property and the Power of Eminent Domain* [1986]): the proper division between state power and individual right in a constitutional democracy. Epstein places the citizen of such a democracy in the position of bargainer with the government. The role of the state is to maximize the gains from economic activity. But in maximizing gains, the state must make many decisions about how the gains are to be divided across society. In so doing, the state exercises its coercive power over individuals by placing prices on their activities. For example, states may subject out-of-state corporations to unfavorable tax treatment in exchange for the right to do business with the state. Or the state may ask citizens to compromise their religious beliefs in order to qualify for unemployment compensation.

Epstein believes that people have absolute rights, that constitutional government entails limits on sovereign power, and that the status quo, or the common law regime, establishes a set of individual entitlements that the state cannot take away at its whim. When these rights are abridged, he argues, the state must either pay compensation or show that its act was designed to prevent wrongful conduct.

Epstein sees the doctrine of unconstitutional conditions as a generalized principle that encompasses the takings clause as well as other provisions of the Bill of Rights and the Fourteenth Amendment. Unconstitutional conditions doctrine holds that the state may not force private actors to give up constitutional rights as a condition of being permitted to do something that the state is otherwise empowered to prohibit. For example, at the turn of the century states were entitled to exclude out-of-state corporations from doing business within their borders. Nevertheless, the Supreme Court held that states could not

grant access to out-of-state corporations on the condition that the corporations waive their right to diversity jurisdiction in federal court, thus relegating their disputes to the state tribunals.

The law of unconstitutional conditions presents a paradox. First, the state can prevent the activity altogether. Second, because what it is offering is a bargain, acceptance makes both parties better off; as a result, the corporation in the above example is at least as well off as it would be had the state excluded it altogether, or it would not accept the bargain.

Epstein tries to resolve the paradox with this principle: it is insufficient for the state to show that all affected parties are better off than they would have been had there been no bargain at all. Rather, the state must also show that no one is worse off than he or she would have been had the state imposed an "optimal" set of social exchanges. The victim might not appear aggrieved if the base line is what the state might have done; but the victim is aggrieved if the baseline is some alternative, optimal regime. Considering a decision such as *Lochner v. New York* (1905), in which the Court struck down a statutory ten-hour limit on bakers' working days, the labor market is competitive and competition creates the optimum state of affairs. The aggrieved person is either the employer, or the employee wishing to work more; the statute, therefore, is unconstitutional and compensation is due.

Writing of cases decided during the substantive due process era (roughly 1900–37), Epstein makes clear how many decisions were cast in unconstitutional conditions language, even though they were technically classified under other constitutional headings such as substantive due process. Writing mainly about the Warren Burger and William Rehnquist courts, he finds the same thing repeated, although the cases were often classified under the takings clause or the First Amendment.

The strict doctrine of unconstitutional conditions that Epstein proposes leaves him little room for most of the regulatory state, most types of progressive or discriminatory taxation, and many restraints on individual liberty.

HERBERT HOVENKAMP
University of Iowa

C. EDWIN BAKER. *Advertising and a Democratic Press*. Princeton: Princeton University Press. 1994. Pp. xii, 203. \$24.95.

This analysis raises to a new level scholarship on advertising as a means of financial support for media, perhaps the oldest, most familiar and enduring concern about the United States media. Whereas the literature has generally focused on the *what*, C. Edwin Baker focuses on *why* the advertising role is unhealthy. He departs from previous scholarship that both relied on anecdotal "horror stories" of influence on content and assumed the reasons advertising

was bad for media required no elaboration beyond the familiar generalization (often only implied) that its very role in financing news delivery cast doubt on the quality of the news delivered.

Baker advances scholarship, then, first with the level of inquiry sustained. Second, whereas some begin from the premise that advertising undermines "objectivity" of the news, the author, although not the first to "deconstruct" objectivity, argues that efforts to "maximize advertising revenue contributed to the development of objectivity as a ruling ideology in journalism" (p. 57), ideology that has become part of the rationale for perpetuating the advertising role. Third, and most important, Baker avoids usual variations on a theme of simply questioning the advertising role and goes straight to the logical basis of a concern in the first place, declaring that the role negates a free and democratic press.

Baker thus challenges earlier works that implied that the role of advertising merely means journalists must prevent tainting of news, scholarship that assumes advertising must be accommodated and professional vigilance can prevent any pernicious effect. Declaring his book's main purpose is "to evaluate the assumption that advertising plays a key structural role in maintaining a free, democratic press" (p. 5), Baker asserts a truly free press is impossible because "advertising operates as a social control mechanism that is often more effective than government censorship in limiting press freedom" (p. 56). And democracy is negated because advertiser needs drive media content, which "is very different from the political and cultural diversity essential to a healthy democracy" (p. 58).

The book advances scholarship by stressing the need for structural change to solve a problem too complex for professional standards to contain. The first three chapters, respectively, cite structural effects of advertising on the newspaper industry; detail four ways advertising affects content (stressing that it primarily subsidizes content for the affluent and refuting the claim of "separate" editorial and business sides); and challenge assumptions that "the media market leads to 'efficient' results" and that "'efficiency' is a proper goal of production and distribution" (p. 72). In chapter four, the book's centerpiece and most controversial part, Baker emphasizes legislative action in examining several "policy proposals" as alternatives for supporting media. The final chapter, "The Constitutionality of Taxation or Regulation of Advertising," argues those proposals produce no constitutional conflict.

An important addition to mass-communication scholarship, the book is not a historical account, although historians will find of interest the forthright dissection of a longstanding media issue. Neither anti-advertising nor antimedia, the book does not advocate eliminating advertising support. Rather, it appeals for reform through structural change to eliminate both government and commercial influ-

ence on media and argues the appropriate reform leaders are journalists, whose "support is likely to be crucial" to achieving such change (p. 137).

HAZEL DICKEN-GARCIA
University of Minnesota,
Twin Cities

RICHARD H. K. VIETOR. *Contrived Competition: Regulation and Deregulation in America*. Cambridge: Belknap Press of Harvard University Press. 1994. Pp. 439. \$35.00.

In this volume Richard H. K. Vietor addresses a subject of both current interest and historical significance. Americans trying to select a long-distance telephone service, purchase an airline ticket, or open an interest-bearing checking account all rub up against the world of business regulation (and deregulation) that Vietor seeks to understand. Indeed, the creation in the 1930s of a complex regime of micro-economic stabilization in key industries and its subsequent dismantling in the 1970s and 1980s constitutes one of the major episodes in modern U.S. business history.

Vietor examines the process of regulation and deregulation from a managerial perspective, and that fact sets the book apart. An introductory overview describes the origins and nature of the New Deal's regulatory apparatus and its evolution over time, but the heart of Vietor's analysis lies in four case studies of how individual firms in various regulated sectors—American Airlines in transportation, El Paso Natural Gas in energy, AT&T in communications, and Bank-America in finance—responded to the flood and ebb of regulatory tides. A summary chapter explores the implications of his findings for scholars interested in the theory of regulation and for business executives intent on learning how best to adapt to regulatory change.

Unifying Vietor's case studies is the central insight that "the real significance of regulation . . . is its impact on market structure" (p. 310). Under the New Deal regulatory regime, markets tended to be service intensive, with high cost overhead. Resulting inefficiency was partially offset by widespread availability, price stability, and the perception of pricing equity. But in the end the New Deal order was unable to respond adequately to macroeconomic and technological change, to the challenge of new ideas, or to the assaults by political and business entrepreneurs who for a variety of motives sought to alter it. By the mid-1970s, the perceived failure of existing regulatory arrangements had made deregulation an idea whose time had come. As deregulation unfolded, however unevenly and incompletely, markets were once again redefined, with industry boundaries determined less by regulators and more by technology, customer demand, and supplier economies. Markets became segmented in new ways, and pricing mechanisms became infinitely more complex. In general,

prices declined but so did service, at least when gauged by the criteria of the earlier regime. In telecommunications, deregulation led to more competitors, but elsewhere it brought renewed integration and concentration.

Overall, the book has much to recommend it. Vietor's research is truly prodigious, his mastery of the intricacies of both government regulation and business maneuver is impressive, and his managerial perspective is illuminating. The result is a stimulating view of modern business regulation. Ironically, the volume's virtues also contribute to its chief defect. The case studies are difficult to follow because the wealth of detailed information overwhelms both the analytical framework and the reader; often there are too many trees and not enough forest. Consequently, the study will probably not gain the wide audience the subject deserves, but it will undoubtedly serve as must reading for specialists.

ROBERT M. COLLINS
University of Missouri,
Columbia

ROBERT F. BURK. *Never Just a Game: Players, Owners, and American Baseball to 1920*. Chapel Hill: University of North Carolina Press. 1994. Pp. xv, 284. \$34.95.

This is the first of a projected two-volume history of player-management relations in professional baseball by Robert F. Burk. This modestly researched study covers the first fifty years of professional baseball. Burk describes the evolution of the social origins and career of the professional ballplayer, particularly his ethnicity, economic situation, and battles with management. He examines how management tried to control the labor force by limiting access to employment and restricting choice of employer and bargaining leverage through the reserve clause. He asserts, unconvincingly, that playing rules were often changed expressly to curtail players' statistical accomplishments and thereby keep down their salaries. Burk also scrutinizes how players individually and collectively sought greater control over the workplace and higher salaries. He points out how players took advantage of league mergers and trade wars to improve their positions. Efforts at unionization were short-lived, however, because of internal divisions among players and because management exploitation undercut efforts at collective bargaining.

Burk is generally faithful to the scholarship on baseball, although he would have benefited from reading Benjamin Rader's recent synthesis, *Baseball: A History of America's Game* (1992). When he ventures into new territory, beginning with his comparison of the Knickerbocker baseball club to their "Puritan ancestors," he is on shaky ground. Burk's research was not very extensive, largely limited to a summer at the National Baseball Library in Cooperstown. There are few citations from *The Sporting News* or the extensive contemporary periodical literature, and limited

use is made of the valuable papers of August Herrmann, long-time National League president.

Burk has compiled a number of tables charting players' annual salary averages that present a false sense of precision. His data in the tables are drawn exclusively from published sources that have just scattered information on salaries. He does not use the semimonthly accounts for some of the mid-1910s for the New York Yankees and Philadelphia Phillies found in the George Weiss Collection at Coopers-town. Elsewhere Burk exaggerates minor league wages by inflating the length of their playing season.

Burk relies heavily on the Lee Allen Notebooks for his analysis of players' education and ethnicity. Yet he misuses this valuable source. He argues that 43 percent of the players in the early 1900s attended college (prior scholars have estimated about 25 percent) because he disregarded men for whom Allen had no data. Had Burk been more familiar with baseball research, he would have known that virtually all major leaguers who attended college have been identified, and he should not have discounted those whose educational levels were not reported. Similarly, Burk's ethnic analysis is flawed because he does not indicate how he dealt with ballplayers for whom no ethnic data are reported. I suspect he undercounted players of WASP backgrounds since most unidentified individuals in the Allen Notebooks had British surnames. Yet players whose names ended in a vowel were nearly always identified. Hopefully Burk's next volume will be more judiciously researched and analyzed.

STEVEN A. RIESS
Northeastern Illinois University

CLAY MCSHANE. *Down the Asphalt Path: The Automobile and the American City*. (Columbia History of Urban Life.) New York: Columbia University Press. 1994. Pp. xvii, 288. \$29.50.

In this wide-ranging, compact account of the prehistory and infancy of the car culture, Clay McShane argues against the technological determinist idea that the automobile created suburbia and our contemporary culture of speed, danger, and consumption. Instead, McShane deploys material on subjects as various as paving techniques, vehicle regulations, popular songs, street riots, city plans, and magazine advice columns to demonstrate that the culture of suburbanization preceded, and made possible, the popular adoption of internal-combustion automobiles. After all, McShane explains, engineers had known how to make street-worthy steam-powered vehicles since the early part of the nineteenth century. Although the public rightly understood steamers' unfortunate tendency to explode, the problem was less the vehicles than the roads, or more precisely, common attitudes toward streets. Working city folk did not want to pay for smooth pavements or to abandon their places of public life to fast-moving, rich

people's pleasure vehicles. Average Americans had to learn to see their streets not as places to socialize or as open areas permitting light and air in otherwise choked cities, but instead as arteries for traffic. They had to learn, as they did in the years between 1900 and 1917, to identify mobility with progress and with their own interests, to accept "the utopian visions that diminished resistance to the automobile" (p. 124), in order to make the laws, finance the pavements and the plans, and buy the cars that revolutionized urban America.

McShane adroitly handles culture and technology, and the complicated relations between the two. He is at home exploring the sometimes contradictory nuances of urban reform politics, the advantages and disadvantages of granite block, cobblestone, macadam, and asphalt pavements, and the history of street safety practices. Unlike many historians of technology, McShane also examines the historical significance of race, class, and gender for the making of the car culture, offering some suggestive analyses, for example, on the relationship between male dominance and automobility. Although I might have interpreted some of the women's history material rather differently, and would have liked to see him push further such questions as the gendered dimension of jitney travel, I applaud his willingness to take social forces seriously.

McShane ends by examining the ways architect Daniel Burnham and other speed-loving, affluent white male drivers reconfigured cities in order to achieve the suburban ideal and make urban America safe for life in the fast lane. McShane concludes that pro-auto Progressive era planners knew the costs of what they were doing, but went ahead and did it anyway, and produced the fragmented, decaying, dangerous cities we live in now. I would have liked to see him work these ideas out a little less hastily and synthesize his wealth of research more fully. Still, no other automotive history does as much to bring the nuts and bolts of urban life together with the human aspirations and activities of those who live it. This is an indispensable book for anyone interested in the history of the car culture.

VIRGINIA SCHARFF
University of New Mexico

RICK KENNEDY. *Jelly Roll, Bix, and Hoagy: Gennett Studios and the Birth of Recorded Jazz*. Bloomington: Indiana University Press. 1994. Pp. xx, 233. \$24.95.

Rick Kennedy's book relates the history of the Richmond, Indiana, based Starr Piano Corporation's Gennett Studios, established in 1915. Despite a title emphasizing jazz, Kennedy actually explains the production of a wide range of recordings, including those for Ku Klux Klan members, country and blues artists, and mail-order budget labels. Kennedy provides an interesting overview of Gennett's evolution by using interviews collected by John MacKenzie from

the Indiana Historical Society, as well as interviews of his own. Kennedy shows how the technological expertise and tastes of individuals, such as entrepreneur Fred Gennett and employee Fred Wiggins, shaped the "small Indiana label," which Kennedy calls a "Rosetta stone for early 1920s jazz" and a "lightening rod" in "American folk music" (p. xvi).

Kennedy's primary concern is to explain the historically important role Gennett recordings played in the careers of jazzmen like Jelly Roll Morton, King Oliver, Hoagy Carmichael, and Bix Beiderbecke. Gennett Studios was not as influential as larger recording companies such as Victor, despite Starr Piano's victorious patent suit in 1921 against Victor Talking Machines, which assured that lateral cut recording technology would be available for the public domain. Kennedy persuasively makes the case that Gennett helped artists make debut recordings, but he does not tell us much that is new about jazz recording history generally. Kennedy does provide a valuable sense of how technical and business decision making at the local level determined much of what has been left to posterity in the rare Gennett discs, which are highly valued by many collectors.

Kennedy documents a variety of Gennett marketing strategies. Country, or "hillbilly," music was popular with rural consumers, for example, who often relied on mail-order catalogues. Thus, Gennett produced a cheaper line of records based on its masters for Sears Roebuck and encouraged performers to attend recording sessions in southern towns. Gennett recorded Gene Autrey and Bradley Kincaid, among many others. Kennedy is not always able to determine the royalty policies of Gennett, and he raises questions throughout the text about the fairness of the company's remunerations to musicians.

Kennedy's combination of Starr Piano Corporation history with the development of jazz and other popular music from the 1920s is a novel approach to this topic. Kennedy has made good use of interviews, although expanded discussion of how and when these interviews were collected would strengthen the methodological value of this study. Similarly, scholars may find some of Kennedy's observations underdocumented. Because Kennedy establishes that Gennett was important for some midwestern territory bands, one wishes he had incorporated more of the recent scholarship on these groups. But Kennedy makes good on his promise to illustrate that "few record companies in the 1920s documented America's musical grassroots as thoroughly as Gennett Records" (p. xvi).

KATHY J. OGREN
University of Redlands

SANDRA HAARSAGER. *Bertha Knight Landes of Seattle: Big-City Mayor*. Norman: University of Oklahoma Press. 1994. Pp. xv, 334. \$28.95.

Bertha Knight Landes, the first woman elected mayor of a major American city, is one of a score of forgotten women who garnered national headlines in the 1920s and then temporarily vanished from the historical record. Like so many others, her contributions were first retrieved in Janet James and Edward James's *Notable American Women* (1971). Her achievements were more fully recognized in Doris H. Piereth's article on Landes in Karen Blair's *Women in Pacific Northwest History: An Anthology* (1988) and in Blair's reflection that Landes's ability to translate her success in the women's sphere to real power as mayor of Seattle was the "culmination of generations of women's dreams." In undertaking this full-length biography, Sandra Haarsager faces a daunting task. Like so many of her successful sisters, Landes left few papers, specifically two boxes of letters, clippings, and correspondence, and four short articles. Haarsager's approach to this paucity of sources is to emphasize context, arguing that Landes is significant "for what she embodied as well as what she accomplished" (p. 284).

Through her meticulous research in local and organization archives, Haarsager shows the Massachusetts-born Landes as a woman rooted deeply in nineteenth-century values of family and duty while also representative of the first generation of college educated women (the sister-in-law of David Starr Jordan, she earned her A.B. at Indiana University in 1891 when he was president). After a brief career in teaching, she married geologist Henry Landes and assumed the role of faculty wife at the University of Washington and full-time mother. In her thirties at the turn of the century, she began her redoubtable career as clubwoman and Progressive reformer. By 1922, she had led Seattle's Century Club, the Seattle Federated Women's Clubs, and the Washington State League of Women Voters in promoting the welfare of children and women in "the larger home," prompting a newspaper comment that she was "probably the best known clubwoman in Seattle" (p. 40). With the support of her husband, the backing of organized women, and a campaign that insisted that home standards be city standards, Landes became the first woman to win a seat on the city council (another less-heralded woman candidate won also). Two years later, in a brief stint as acting mayor, she made national headlines by sacking the police chief. Running on a law-and-order and municipal housekeeping platform that emphasized not only regulating cabarets and cracking down on bootleggers but also efficiency and economy in government, Landes campaigned for a city manager plan even as she ran for mayor. Backed by women and aided by the decision in a notorious bootlegging case the day before the election, Landes, victorious at the age fifty-seven, was ready for the "romance" of building Seattle. Unlike so many other women officeholders of the 1920s, Landes was neither widow nor daughter, but had won on her own merit and record. Her support of public

power and health and child welfare provisions, her defense of the environment, her strict law enforcement campaign, and her fiscal rescue of the troubled municipal street railway earned her high marks in good government and also produced a coalition of enemies that led to her surprising defeat by an unknown theater manager in 1928.

Haarsager's detailed analysis of the campaigns and administration of Landes provides a good case study of the complexities faced in building and sustaining coalitions by women once they achieved the rare political office in the post-suffrage 1920s. She concludes with a lengthy chapter examining Landes's life in terms of Michel Foucault's power/knowledge link and the liminality theory advanced by Victor Turner. Here the elusive flesh-and-blood Landes, who emerges only fleetingly with quotes like "there's more to being a mayor than just playing around with Queen Marie and Lindbergh when they come to town" (p. 180), is finally and unnecessarily drowned in context and connections.

DOROTHY M. BROWN
Georgetown University

H. LARK HALL. *V. L. Parrington: Through the Avenue of Art*. Kent, Ohio: Kent State University Press. 1994. Pp. xvii, 360. \$39.00.

Vernon Louis Parrington's landmark study *Main Currents in American Thought* (1927) helped establish an important subfield of American history. The two volumes, *The Colonial Mind* and *The Romantic Revolution*, were supplemented by the incomplete and posthumous work *The Beginnings of Critical Realism in America* (1930). Together these works define Parrington's significance for American intellectual history. Parrington and the often polemical critical perspective he brought to his subject have made him a widely studied intellectual figure in his own right. Now we have the first biography of Parrington himself.

H. Lark Hall has given us an immensely detailed and informative portrait. In traditional manner, she proceeds chronologically. Parrington, born in Aurora, Maine, in 1871, inherited a bit of Yankee radicalism from his grandparents, but grew up amid a more conservative midwestern Republicanism after his family's migration to Texas and then to Kansas. A farm boy intellectual, Parrington spent as much time as work permitted reading in the Emporia public library preparing for his entrance to Harvard. Hall then traces Parrington's academic career at Emporia College (1893-97), the University of Oklahoma (1897-1908), and the University of Washington (1908-29).

This book's subtitle provides the key thematic framework for Hall in studying Parrington. When he wrote out an "autobiographical sketch" in 1918, Parrington underscored the particular nature of his radicalism. "My economics must be sugar-coated for

me," he said; that is, he must enter the field of modern social thought "through the avenue of art" (p. xii). Parrington took a long route to arrive at the radicalism that frames *Main Currents*. His varied teaching assignments covered expansive fields of European literature, until at Washington he became a pioneer of the academic curriculum in American literature. But as Hall ably shows, it was John Ruskin and especially William Morris who helped Parrington pave his own avenue from the literary to the social.

Parrington confided to his diary in 1896 that "all my thoughts of late have run on the money question" (p. 69), and increasingly we learn of his distaste for the "crude and selfish materialism" that he saw ruining American life (p. 149). But Parrington's radicalism never became theoretical. Aesthetic judgments more strongly informed his opinions, despite the criticism that Parrington's class feelings overrode these judgments in *Main Currents*. Parrington wrote a fair amount of poetry, much of it evocative of the prairie landscape, and he left behind an unfinished novel.

Parrington tried to give a populist ingredient to his radicalism. He later considered Harvard a liability for democracy and worked consciously to surpass his Harvard mentor, Barrett Wendell, by discovering a western voice in American literary history. But, however much Parrington enjoyed talking with ordinary people and coaching the Oklahoma football team, in dress and manner he cut a visible aristocratic figure. And he never wholly overcame the extended New England culture that followed him to the Midwest. He set comfortably in an America informed by "the Mayflower ideals" and did not adjust easily to the "ill-smelling hordes of Poles and Italian[s], Russians and what not" who were spreading their "strange ways and strange doctrines" in the new pluralistic United States (p. 85).

Hall states that she intended to write a biography and not to take up all the extended critical discussions of Parrington. One might like to have seen more of the latter, but in her announced purpose, Hall has unquestionably succeeded.

J. DAVID HOEVELER, JR.
University of Wisconsin,
Milwaukee

JOEL WILLIAMSON. *William Faulkner and Southern History*. New York: Oxford University Press. 1993. Pp. 509. \$35.00.

Joel Williamson's study is the one book on William Faulkner that historians should read. There are other fine biographies available, of course—by Joseph Blotner, Cleanth Brooks, David Minter, Irving Howe, Frederick R. Karl, and many others—but Williamson outshines them all in re-creating the distinctly southern past that Faulkner inherited and then transformed into three or four of the greatest novels of all time. Like the master himself, Williamson is en-

tranced with the world of Faulkner's forefathers, the rough, racist, violent Mississippi of the late nineteenth century. "Ancestry," making up the first third of Williamson's book, sits authoritatively on a mountain of research in county courthouse records, letters, newspapers, and musty documents of every sort, including family rumors and gossip. Each reveals "a truly historical Yoknapatawpha County," Faulkner's richly imagined world. "This is not the world that William Faulkner created; it is, rather, the world that created William Faulkner, and it encompasses the universe of race, class, sex, and violence, of family, clan, and community that affected him so profoundly and about which he wrote with such telling effect" (p. 124).

No one familiar with Williamson's *The Crucible of Race* (1984), a seminal, if highly idiosyncratic and controversial study of "Black-White relations in the American South since emancipation," will be surprised to learn that race in all its many faces figures prominently in his discussion of Faulkner. Faulkner's youth was saturated with virulent racism; Mississippi routinely led the nation in the lynching of blacks. Williamson vividly re-creates a savage lynching in Oxford in 1908, when the impressionable Faulkner was eleven. The barbaric event would serve, Williamson surmises, as the source for Faulkner's acclaimed short story, "Dry September," and the murder of Joe Christmas in the masterpiece, *Light in August* (1932). All of Faulkner's literary contemporaries in the South—white and black—grew up with such searing experiences, but in the long middle section titled "Biography," Williamson writes with tunnel vision about his subject. This is regrettable because comparisons and specific comments on Faulkner's generation would have given his book a broader, richer intellectual and literary context.

But Williamson never shrinks from looking critically at Faulkner and his world. He speculates that there is every possibility that the writer's slaveholding ancestors, although not of the great planter class, fathered several black Falkners (the "u" was added later). Faulkner made miscegenation (along with incest) one of the major themes in his novels, many of which are indirect critiques of racism. Yet, as Williamson and various other biographers make clear, Faulkner was too much a traditional white male of his era to stay in step with the civil rights movement. Worse, his attitudes hardened and, on one infamous, highly publicized occasion, he talked like a fool about whites and blacks: "But if it came fighting I'd fight for Mississippi against the United States even if it meant going out in the street and shooting Negroes." On the race issue, Williamson writes, after chronicling his descent from moderate liberalism, Faulkner "was becoming ridiculous, a fact that could not have been lost on him" (p. 308).

Williamson concludes with a daunting task: an extended essay on "A Faulknerian Universe." This he attempts after wryly acknowledging that "any asser-

tion at all" about Faulkner's work will "justly bring down upon one's head" a mighty band of "professional Faulkner scholars" (p. 355). No doubt his notion that Faulkner's world view is stretched between the continuums of idealism/realism and nature/society is simplistic. Never mind. Williamson's probing of Faulkner's relationship to southern history has something to teach even the mightiest oaks in the groves of academe.

BRUCE CLAYTON
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MICHAEL WRESZIN. *A Rebel in Defense of Tradition: The Life and Politics of Dwight Macdonald*. New York: Basic Books. 1994. Pp. xviii, 590. \$30.00.

In recent years, Dwight Macdonald has joined Theodor Adorno and Max Horkheimer as a whipping boy for academic enthusiasts of popular culture. Denunciations of Macdonald's postwar critique of mass culture are such obligatory features of current scholarship in cultural studies that younger readers may be forgiven if Macdonald's name conjures up a Hollywood image of the professor in a smoking jacket, calmly sipping a glass of sherry while preparing for a graduate seminar. At the very least, Michael Wreszin's lively biography should dispel the caricature of Macdonald as an academic snob by restoring him to a place of honor in twentieth-century American radicalism. Wreszin rightly puts Macdonald's career as editor and writer for *politics* during the period 1944–49 at the center of his narrative and argues for Macdonald's significance in a lineage of democratic critics of liberalism that includes Randolph Bourne and the authors of the Port Huron statement.

Wreszin's book is the first biography of Macdonald, the leading "New York intellectual" who lived from 1906 to 1982, and it is clearly a labor of love. Wreszin deeply admires "Dwight" (as he refers to him throughout) for his humor and freewheeling personal style, and his book makes extensive use of materials in the Macdonald Papers at Yale University to capture its subject's unique sensibility and personal relationships over time. Above all, Wreszin excels in detailing Nancy Rodman's role in Dwight's radicalization. Nancy initiated Dwight into New York's Marxist Left before their marriage in 1934, helped keep *Partisan Review* afloat as managing editor during Dwight's tenure on the editorial board, and later encouraged his turn from *Partisan Review* and Trotskyism to the Bournean radicalism of *politics*. The "soul of *politics*," as one contributor called her, Nancy organized the magazine's "packages project" to aid antifascist intellectuals in war-ravaged Europe and set an example of a humane politics of conscience (p. 136).

Wreszin also vividly portrays the Bohemian community the Macdonalds presided over in New York and Cape Cod, where entry depended as much on a commitment to public nudity and heavy drinking as it

did on a willingness to engage in political conversation ("Eileen," Macdonald snapped in the midst of one such gathering on the Cape, "we're not discussing anything!" [p. 183]). In the dramatic high point of the book, Wreszin offers a harrowing account of Macdonald's near-breakdown in the five years after *politics* folded in 1949, as Dwight's marriage to Nancy dissolved, his hopes for a "third way" between the Soviet and American camps evaporated, and the *politics* community of European and American intellectuals fractured along Cold War lines. The result was a simultaneous personal, political, and professional crisis of identity that he only partly resolved with his second marriage to Gloria Lanier in 1954 and a turn to cultural criticism in the *New Yorker* and other upscale venues. For the rest of his life, Macdonald was plagued by doubt that his later work failed to meet the standards of *politics*, and he was paralyzed for his last decade by an almost total writer's block.

Wreszin by no means minimizes Macdonald's personal flaws and offensive behavior—his cruelty to his two wives, his verbal sadism toward his sons, and his insensitivity about anti-Semitism in conversations with Jewish friends—but he generally upholds his subject as an exemplar of the generous humanism that inspired *politics* at its best. The cost of such sympathy is that it often comes at the expense of a critical interrogation of Macdonald's ideas. Wreszin expertly navigates the precarious course of Macdonald's anti-communism in the late 1940s, when the former radical pacifist echoed Americans for Democratic Action in his attacks on Henry Wallace and other Left-liberal critics of the Cold War. But he is too willing to say that Macdonald's "honesty" and willingness to "take a stand" excuse everything he did, whether the issue is his hyperbolic attack on "an encroaching cultural totalitarianism" in the late 1950s or his refusal to see a contradiction between his critique of mass culture and his newfound status as a celebrity intellectual on television and radio (p. 285).

"The major consistency in [Macdonald's] life was paradox and contradiction," Wreszin writes of the man whose passions included literary Modernism, Trotskyism, anarcho-pacifism, anticommunism, the campaign against "masscult" and "midcult," and the New Left (p. xiv). As the book's title suggests, Wreszin attempts to give that "paradox and contradiction" some coherence by viewing Macdonald as an American traditionalist, whose patrician upbringing gave him the confidence to challenge the wartime state and consumer culture. But this effort to make Macdonald a latter-day Henry Adams is ultimately unconvincing. Edmund Wilson and Robert Lowell played that role far better than Macdonald, who could never explain how a radicalism grounded in personal values would determine which values were worth respecting in an age of doubt. Macdonald treasured Simone Weil and Dorothy Day for their spiritual resistance to modernity, but he lacked their religious discipline. Neither could he embrace the uncompromising tra-

ditionalism of his admirer and correspondent, T. S. Eliot. Macdonald was in the end loyal to the artistic Modernism of the early twentieth century and the cosmopolitanism it inspired in the American intelligentsia. It was those commitments, far more than any WASP heritage, that fed his fury at mass culture in the 1950s, and at the Jewish and African-American nationalisms that outraged him in the 1960s. And it was those commitments that proved so difficult to reconcile with the localistic radicalism Macdonald had tried to revive in *politics*.

Macdonald performed his greatest service to American culture as a Modernist dissident from within the Left-liberal intelligentsia. Like his favorite modern writers—Ezra Pound, James Joyce, and Eliot—Macdonald believed that a debased public language and the horrors of industrialized warfare had created a vacuum of meaning in twentieth-century life. The central metaphor of his best criticism was the thoughtless machine that destroyed human bodies, language, and culture without any redemptive purpose: "the maximum of physical devastation accompanied by the minimum of human meaning," as he described World War II (Macdonald, *Memoirs of a Revolutionist* [1958], p. 96). This denunciation of society as a mindless automaton foreclosed any careful weighing of practical possibilities for political change. But it also inspired Macdonald to champion individual responsibility and honest language within leftist movements given over to sociological reductionism and sloganeering. He continued to play such a role until close to the very end. The critic who excoriated "lib-lab" apologists for aerial bombing of civilians in World War II appeared at the 1968 "counter-commencement" at Columbia to defend the autonomy of the university against radicals calling for "two, three, many Columbias." Those who sought to destroy the university would find, he warned, that "they would have nothing to replace it" and that their actions would only "stimulate the already oppressive standards in the nation." His words provoked what Wreszin calls "a chorus of cheerful booing and hissing" (pp. 455–56). The chorus has since lost its cheer, which is all the more reason to welcome this vivid biography of the American Left's Modernist conscience.

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THOMAS E. BLANTZ. *George N. Shuster: On the Side of Truth*. Notre Dame, Ind.: University of Notre Dame Press. 1993. Pp. xi, 479. \$34.95.

Born in 1894 in Wisconsin of a German-American family whose forebears had emigrated to America in 1848, George Nauman Shuster became a nationally known journalist, government official, and college administrator. Educated in parochial schools, Shuster graduated from the University of Notre Dame, and in

1917 he enlisted in the U.S. Army. After serving for six months in France, he returned home and accepted a position on the small English faculty at Notre Dame.

In 1924, Shuster left Notre Dame for New York to enroll in the doctoral program at Columbia University. While teaching at St. Joseph's College for Women in Brooklyn, he also began writing for *The Commonweal*, a journal designed to bring progressive new social and economic ideas to middle-class, college-educated American Catholics. Shuster spent twelve years as an editor of the journal when it was publishing the views of distinguished Catholic intellectuals on such subjects as Darwinian evolution, the teachings of Pope Pius XI on the living wage, and the opinions of Father Charles Coughlin on world banking. When subscribers objected to his criticism of Generalissimo Francisco Franco in 1937, Shuster left *The Commonweal* and spent a year studying in Europe.

When he returned to America, Shuster was invited to become president of Hunter College in New York. For the next twenty years he directed the college successfully through changing times, providing new programs for women, adapting to wartime demands, and responding to the pressures of the postwar anticommunist hysteria. In addition to holding a number of government posts during the 1950s, he became an active member of UNESCO and later served as land commissioner for the largely Catholic province of Bavaria. In 1961, Shuster was invited by Theodore Hesburgh to return to Notre Dame as assistant to the president. Here he spent the remaining sixteen years of his life organizing projects and developing programs to enhance the academic prestige of his alma mater.

Thomas E. Blantz has produced a laudatory biography of a noted Catholic educator and statesman. The life of George Shuster, however, is significant not only as the record of a single individual but also as a microcosm of a whole generation of American Catholic intellectuals who wanted to restore serious scholarship and artistic creativity to a society that had settled for intellectual mediocrity and cultural conformity. By cataloging Shuster's impressive record of honors and achievements, Blantz has neglected to explore the important influences on his formative years in New York City during the 1930s, which Shuster once described as perhaps the most satisfying period of his life. There is no mention of any contacts between Shuster and the pioneer social activist, John A. Ryan, who was a member of *Commonweal's* editorial board, nor is there any evidence of close association with Dorothy Day and her influential Catholic Worker movement of the same period. Although Blantz provides listings of Catholic artists, writers, and intellectuals who contributed regularly to *Commonweal*, he offers no real analysis of their overall impact on the journal's readership or their influence on the mindset of Shuster himself.

Now that we have this well-written and carefully

researched biography of the man, hopefully we can anticipate another work that will provide us with a more analytic treatment of those early years that shaped the ideas not only of Shuster but also of so many of his fellow "Commonweal Catholics."

THOMAS H. O'CONNOR
Boston College

MARIO T. GARCÍA. *Memories of Chicano History: The Life and Narrative of Bert Corona*. (Latinos in American Society and Culture, number 2.) Berkeley and Los Angeles: University of California Press. 1994. Pp. xviii, 369. \$30.00.

Some individuals are notable because they exemplify the historical past. In Chicano history, Bert Corona is such a person. The life story of this truly remarkable leader is the essence of Mario T. García's study.

The book covers Corona's early life in El Paso, Texas. Here the Mexican Revolution, overt racism, and witnessing the misery of the Great Depression shaped Corona's commitment to social justice. In Los Angeles, the political consciousness of Corona blossomed further through his efforts as a labor organizer with the CIO and as president of Longshoremen's Union Local 26. Corona relates that he applied the basic lessons learned from organizing workers to his greater vocation: a commitment to social justice.

The many facets of Corona's public life will hold the most interest for historians. He states, "I have lived a long time and have been a part of a good deal of history" (p. 321). Indeed, he has been on the front lines of virtually every major Chicano and Latino organization in California and the nation. Because this narrative reads as an eyewitness inventory and assessment of the major personalities, organizations, and important periods of Chicano history, the book will command the attention of Corona's contemporaries and historians alike.

García has done a masterful job of guiding the chronicle through the focal points of Chicano history. Writing with an intimate knowledge of history, García provides a personal portrait of Corona's background and creative development, as well as the events, personalities, and forces that shaped his intellectual life. Although sympathetic to Corona, García does not engage in myth-making. Instead, the effect of the book is powerful because it reads as a collective social and political history of Chicanos.

As a personal testimony, this book contains some vivid and provocative descriptions of Corona's association with the Mexican-American leaders of the 1940s and 1950s, key figures of the Chicano movement of the 1960s, as well as presidents John Kennedy and Lyndon Johnson, and senators Robert F. Kennedy and Hubert H. Humphrey. Corona's account of events, however, is sometimes open to question. For example, in describing the Mexican American Political Association (MAPA), he recalls an early invitation to meet with Chicanos in Pasco, Washington, during

the mid-1960s. Corona's first trip was to Yakima, Washington, in 1966, not Pasco. No chapter of MAPA developed in Washington State. Corona's visit to Yakima was followed by the organization of a political group called the Mexican American Federation of Washington (p. 237). This seemingly minor point merely demonstrates that historians will have varying responses to this book.

Without doubt, García's book is an important and timely contribution to Chicano historiography, given the first-person perspective on much of twentieth-century Chicano history. It is also significant because it adds to the dearth of biographies in Chicano literature. For these reasons, the volume will have the attention of historians and general audiences.

ERASMO GAMBOA
University of Washington

SAMUEL WALKER. *Hate Speech: The History of an American Controversy*. Lincoln: University of Nebraska Press. 1994. Pp. 217. Cloth \$25.00, paper \$11.95.

Samuel Walker here discusses how the hate-speech issue has been a bugaboo for rights-conscious Americans since the 1920s and, like other censorship issues, has raised serious questions regarding the permissible limits of free speech. In this debate, freedom has generally been hailed as the rule, except in periods of crisis. As Walker points out in this useful book, in the area of free speech, nothing is ever foreordained. During World War I changes did occur in more formal policy with the beginning of the principle of the free marketplace of ideas, which included, as Oliver Wendell Holmes was to argue later, "not so much free thought for those who agree with us but freedom for the thought we hate." This view came in time to be the majority position on the First Amendment. America's tolerance is remarkable, as almost every other country prohibits hate speech directed at religious, racial, or ethnic groups.

Yet, although tolerance served to protect various "shirts" in the 1930s, Jehovah's Witnesses in the 1940s, both supporters and critics of the civil rights movement in the 1960s, and even Nazis in Skokie in the 1970s, it was not consistent or universal. In the 1950s Cold War tensions led to the curtailment of communist expression, while in the 1960s calls were heard for limiting too-sharp criticism of the Vietnam War. Thus, both the censors and the free-speech advocates have swerved from time to time. The advocates, however, generally enhanced their position through the growing ethnic, racial, religious, and gender tolerance of the 1960s and 1970s. This acceptance, Walker argues, stemmed from the fact that no real political constituency calling for censorship of hate speech has existed in this country. Such minority constituencies as the American Civil Liberties Union, the National Association for the Advancement of Colored People, and the American Jewish Congress have generally agreed that hate-speech laws did not

serve their interests. Even states passing such laws in the 1950s were a decided minority. Illinois' Group Libel Law was repealed by the state legislature that passed it, although the U.S. Supreme Court upheld it in 1952.

A shift came in the 1980s and the early 1990s, with some concerned liberals urging censorship of hateful speech, including pornography. Advocates called for protection from hate speech on campuses, in the workplace, in the streets, in bookstores and newsstands. Campus speech codes were especially troubling. The law professors who drafted these codes provided sophisticated scholarly arguments contending that codes were not inconsistent with the First Amendment, represented the moral and political high ground, and were needed antidotes to the frightening upsurge of campus racism, sexism, and homophobia. Although Walker describes this process well, he never fully explains it.

Gradually, however, campus speech codes came to be seen as repressive, and a counter political constituency of professors perceived the codes' threat to academic freedom and free speech. The courts, coming in time to throw codes out, agreed with the American Association of University Professors that "free speech is not merely one part of higher education but is the very precondition of the academic enterprise itself."

So again free speech was catapulted into the national political arena, with the politics of permissible free speech now having the questionable new twist of liberals advocating curtailment. One can only wish that those opposing the Left's censorship here see not only the virtue of the free marketplace of ideas but are also committed to the attainment of a more sensitive balance in the application of that fundamental American value.

PAUL L. MURPHY
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DONALD FISHER. *Fundamental Development of the Social Sciences: Rockefeller Philanthropy and the United States Social Science Research Council*. Ann Arbor: University of Michigan Press. 1993. Pp. xiv, 343. \$49.50.

It will come as no secret to those who follow current debates about the proper role of the social sciences in the United States that social scientists themselves are a contentious lot. Divided by methodological disputes, they are no less at odds about whether they should function as disinterested scientists or strive to shape public policy. As his entry point into this debate, Donald Fisher, a sociologist, traces the history of the Social Science Research Council (SSRC) from its foundation in 1923 through the 1940s. As the first national institution in the world devoted to the creation of a "science of society," the SSRC, he argues, attempted to shape the development of the social sciences along lines suggested by its patron, the

Rockefeller Foundation. Essentially, he examines the uneasy relationship between a distinguished group of social scientists and the foundation officers who controlled the purse strings, with an eye to explaining how the Rockefeller Foundation attempted to steer social scientists toward "applied" research and away from "pure" research.

In Fisher's view, the foundation's role was always powerful and never benign. Drawing heavily on Marxist theories of the state and on the works of Antonio Gramsci, Fisher's analysis is informed by a class-struggle view of human affairs. Consequently, he depicts the Rockefeller Foundation as an agent of the upper class, bent on imposing the cultural hegemony of the class it served. To support this argument, he discusses at considerable length the various programs, committees, conferences, and scholarships the SSRC funded, arguing that the Rockefeller Foundation's thumbprint can be found on virtually every penny the SSRC spent. In his view, the foundation's constant meddling prevented the SSRC from fulfilling its original goal, which was to foster the "pure" research that would have created a "science of society." Indeed, Fisher explains the development of the social sciences since World War II largely in terms of the role social scientists accepted for themselves under Rockefeller tutelage.

Scholars less enamored of Marx than Fisher will not accept every argument advanced in this book, but many will be interested in his attempt to understand the intellectual and programmatic development of the social sciences by analyzing the relationship between the Rockefeller Foundation and the SSRC. Those who have managed to pursue their careers without benefit of philanthropic grants, however, may question the power he posits in foundations.

JAMES H. JONES
University of Houston

MARILYNN S. JOHNSON. *The Second Gold Rush: Oakland and the East Bay in World War II*. Berkeley and Los Angeles: University of California Press. 1993. Pp. viii, 302. \$35.00.

Marilynn S. Johnson's book is an important study of how the rise of west coast defense industries and a large influx of war workers and military personnel transformed numerous East Bay communities. These East Bay cities, which had traditionally been known as the Oakland metropolitan area, experienced not only sizable population growth but also unprecedented economic expansion, social dislocation, a tremendous demand for housing and social services, and an increase in racial tension. Although Johnson draws on Gerald D. Nash's seminal study, *The American West Transformed: The Impact of the Second World War* (1985), she also explores many new issues, such as the creation of a migrant subculture, the war's impact on East Bay politics, the infusion of southern culture into East Bay communities (particularly the introduction

of the blues and country music), the expanded role of women during World War II, both in the workplace and in the home, and the attempt by established residents to control the public space and to shape the behavior of wartime migrants.

Johnson skillfully illustrates that World War II had a profound impact, in particular, on the economies of East Bay cities, as the defense industry shifted to the west coast. The massive Kaiser shipyards, Marinship, and Moore Dry Dock employed thousands of workers of all races, paid relatively high wages, and afforded many workers their first opportunity at a steady and predictable income in more than a decade. Shipyard employers such as Henry Kaiser, argues Johnson, also established a form of corporate welfare through a series of innovative health and welfare programs, instituted in large measure to increase the efficiency of their employees. Although the health plan and hospitals established by Kaiser are considered significant achievements by most historians, the wartime migrant housing, writes Johnson, resembled "shipyard ghettos" (p. 83). Indeed, World War II resulted in greater housing segregation and racial polarization between blacks and whites in East Bay cities (attributable to an extremely competitive housing market), federally sanctioned racial segregation (which had a profound impact on postwar neighborhood patterns), and virulent white racism.

This is a cogent and well-written book. Johnson's material on the adjustment of migrant families to shipyard employment and East Bay cities, the multitude of changes within the migrant and nonmigrant family unit as a result of the exigencies of World War II, and the evolution of a migrant subculture extends our knowledge and is her most significant contribution. This study falters, however, in several areas. Johnson's primary focus is essentially the cities of Oakland and Richmond, and other East Bay communities receive far less attention and analysis. Although Johnson made excellent use of East Bay newspapers, oral histories, and the voluminous Henry Kaiser papers, she rarely cited the records of the Fair Employment Practices Committee and does not appear to have mined the Bay Area branch files of the NAACP from the 1940s. Moreover, there is only scant reference to the important interracial organizations that were established during the 1940s to eliminate racial discrimination, particularly in housing and employment, and some organizations, such as the Bay Area Council against Discrimination and the California Federation for Civic Unity are conspicuous by their absence. These minor criticisms, however, should not obscure the importance of Johnson's book and its significance to urban and western historians.

ALBERT S. BROUSSARD
Texas A&M University

HAIM GENIZI. *America's Fair Share: The Admission and Resettlement of Displaced Persons, 1945-1952*. Detroit, Mich.: Wayne State University Press. 1993. Pp. xi, 273.

Haim Genizi's career began with an interest in the rescue of Christian refugees by Christian churches and their agencies during the Holocaust. His first work, *American Apathy: The Plight of Christian Refugees from Nazism* (1983), found their performance wanting. For some there was a balm in his findings. It demonstrated that Jewish organizations and rescue agencies might not have been so ineffective as their disunity during the Holocaust suggested. More important, it took the edge off anti-Semitism as the exclusive explanation for America's indifference about the Jewish victims of the Holocaust. In this book Genizi finds that after 1947, spurred by the knowledge that 80 percent of the Displaced Persons (DPs) were Christian, the churches became more active. Yet the anti-Semitism that played a role in shaping the opposition to the admission of Jewish refugees during the Holocaust was not completely dissipated during the postwar years. That opposition was now exacerbated by Cold War hysteria. Initiated by Jewish organizations, the DP Act of 1948 and its amendment in 1950 nevertheless preferred Balts and *Volksdeutsche* over Jews. Jewish defense organizations were chagrined but differed on whether to risk breaking up the interdenominational common front to fight anti-Semitism that had been painstakingly built. So Jewish organizations remained silent, although they were aware that Nazi collaborators and some war criminals were allowed entrance into the United States. Genizi's chapter that details this conflict is the most interesting and relevant in this somewhat labored account of DP politics.

Eventually the original DPs, victims of Nazi policy of genocide, were mingled with a new kind of refugee uprooted by Soviet expansion in Eastern Europe. They would form the majority of those admitted. The entrance of Jewish DPs, however, was not limited by provisions that assigned a high percentage of places to farmers, which Jews were not. The law was so liberally implemented that no group had cause to complain until revelation that a trickle of war criminals had been accepted under its provisions.

Genizi concludes that despite all its shortcomings, America did its "fair share" to relieve the postwar DP problem. That is a balanced conclusion in this well-researched account.

The DP laws are part of the American immigration experience that has been the arena of conflict between restrictionists and liberals since before the turn of the century. The book ends with the passage of the McCarran Walters Act of 1952, which joined the old nativist-motivated restrictionism with a seemingly new kind generated by Cold War hysteria. A perceptive reader will recognize that these forms are related. Had Genizi allowed that law to serve as the anchor of his book, how Nazi collaborators could overnight be converted into patriotic Americans would have become abundantly clear. Anticommunist Cold War hysteria served as the incubator of these conversions.

Finally, a historian faces a special challenge when

narrating the history of the bureaucratic politics of dozens of interacting agencies involved in the DP effort. There is a danger that the narrative will become as turgid as the subject it seeks to describe. This is probably the most thorough account we have of the organizational politics of the DP question, but it does not always meet that special challenge.

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DAVID S. CECELSKI. *Along Freedom Road: Hyde County, North Carolina, and the Fate of Black Schools in the South*. Chapel Hill: University of North Carolina Press. 1994. Pp. xii, 235. Cloth \$32.50, paper \$14.95.

It has been forty years since the U.S. Supreme Court *Brown v. Board of Education* decision declaring legal segregation in public education unconstitutional, and many educators and researchers have commented on desegregation's mixed results for American society. Although the *Brown* decision marked a watershed in the evolution and eventual demise of American apartheid, the unofficial policies and practices of political leaders and public school administrators have resulted in a situation where the proportion of African-American students in urban public school systems who attended all-black or majority-black schools in 1994 was greater than in 1954. Moreover, numerous researchers have pointed out that by virtually any standard for academic attainment used, the quality of the schooling available to African-American students in most urban public schools in the 1990s is poorer than in the 1950s.

From the beginning of the implementation of school desegregation in the late 1950s, some African-American educators and leaders suggested that the black community would have to give up more than it would gain from this highly politicized, educational process. The two major issues for black southerners became the firing or demotion of black teachers and administrators, and the closing of publicly supported black schools. Articles began to appear in educational journals from as early as 1970 describing the plight of the black teacher in public school desegregation, and even earlier campaigns had been launched, spearheaded by the alumni to "save black schools."

David S. Cecelski's book is a fascinating and well-documented case study of the successful year-long school boycott carried out by African-American parents and students to save two historically black public schools. Cecelski presents much convincing evidence that in 1968 the O. A. Peay School and the Davis School not only were essential cultural institutions in this rural and somewhat isolated community but also exemplified the self-determinist activities of southern blacks since Radical Reconstruction. The greater part of the financial support for these schools came from the black community. Building on the work of histo-

rian James D. Anderson, Cecelski also finds that southern blacks were victimized by "double taxation": they paid taxes for the funding of public education and contributed the lion's share of the financial support for the separate black public schools. In the summer of 1968, officials from the Department of Health, Education, and Welfare (HEW) forced conservative whites on the Hyde County School Board to come up with a desegregation plan, and the board ordered the closing of the two black schools and the eventual transfer of all the black students to the (white) Mattamuskeet School.

African-American parents objected but were rebuffed by the school board, so they decided to organize a school boycott. Although the Southern Christian Leadership Conference's Golden Frinks was brought in to provide expert advice and leadership skills to coordinate the protest, it was through the efforts of the grass-roots leadership that the well-coordinated, community-wide campaign was sustained and ultimately successful in keeping the two black schools open. Cecelski has used school board records, HEW correspondence, state police department dispatches and memoranda, newspaper and magazine accounts, and oral interviews to present this first detailed analysis of what can happen when African-American parents and educators decide to take responsibility for the training of black children in the era of so-called public school desegregation.

V. P. FRANKLIN
Drexel University

SALLY BANES. *Greenwich Village 1963: Avant-Garde Performance and the Effervescent Body*. Durham, N.C.: Duke University Press. 1993. Pp. ix, 308. Cloth \$49.95, paper \$17.95.

To use one year (1963) to encapsulate or distill many of the issues that became central to the redefinition of contemporary culture and contemporary politics is a good idea. Sally Banes goes a long way toward convincing us that 1963 was a key to understanding not only our past but also our postmodern contemporaneity. She gives us a powerful array of events, a complete chronicle of life in the fast lane of the early 1960s avant-garde.

She describes many of the new sites of avant-garde production, from dance and new theater, to visual art, film, literature, and happenings. The breadth of the research is wide and deep. The coverage of the alternate cultural field is impressive in its multilayered presentation. It is, in fact, a kind of archaeological stratigraphy of modern times.

The book is divided into seven chapters highlighting the construction, all through the underground culture, of a new cultural paradigm that had to do with the creation of a close-knit artistic community, the collapse of distance between high art traditions and popular cultures, and the discovery of the importance of the simple, the banal, and the mundane as

both an important form of communication and of conflict. Greenwich Village is lovingly described as a complex web of artistic discourses, with local traditions and a reorganized community in which cafés, churches, and community centers played a crucial role in the redefinition of contemporary art and sexual and ethnic identities. Banes leads us with delight into this complex avant-garde network constantly pointing toward signposts of the new understanding of American society characterized by the optimism of the "New Frontier" ideal of the John F. Kennedy administration, in contrast to the old, anguished Abstract Expressionist mood. Now, under Kennedy, a sense of possibilities and of freedom were transferred to the re-experiencing of the body (dance, happening, sexuality), as if youth had finally rid themselves of the dull man with the grey flannel suit.

One of the strengths of the book is the vertical slicing of the cultural field, which allows for some remarkable connections and similarities. One is able to understand how cohesive an avant-garde can be, thus disintegrating the idea of the splendid isolation of art works. Here art is indeed seen in its artistic and social context. This is one of many positive aspects of the book, but at the same time it does not avoid falling into another common trap. Because of the tense focus on New York alone, one has the feeling that everything happened there and only there. It is not even New York but rather Greenwich Village that seems to be the center of the avant-garde universe. Reading the book twice, one realizes that it is in fact the Café Cino which is the heart of the new avant-garde. This focus gives a claustrophobic sense of satisfaction, as if a luxurious intellectual life had a spontaneous generation, without any relation to the outside world. This type of cultural study is interesting but should try to avoid the traditional pitfalls that history and art history have continuously fallen into before: isolation, self-protection, and adulation.

If I were to choose the title of Andy Warhol's "Tuna Fish Disaster" as an entry to Banes's book, it is because I feel it necessary to bring death into her paradisiac narrative. This picture by Warhol symbolically pinpointed the little glitches that often derail the optimism of consumerist society. A bug, here poisoning many happy unsuspecting consumers of tuna fish preserves, betrays the fact that everything was not groovy in paradise and that it really cannot be, even if one wanted "Paradise Now," as the Living Theatre was advocating. What we might get instead is a mirage, Paradise with a worm in the Big Apple, so to speak, or death in our tuna fish sandwich. Things are always more deadly than they appear. Despite some indications that Banes recognizes contradictions inherent in the avant-garde discourse and production, she seems too nostalgic about these happy days for mounting a critical appraisal of it. She is able to understand that many of the avant-garde tics, (discourse of freedom, opposition to the old) were in fact

objectively allied with the recent politics of the New Frontier of the Kennedy administration, but she is unable to push this observation into an ideological critique of the effects of art production. She sees contradictions in avant-garde art but does not try to differentiate the political differences between a resilient, destructive, and anarchistic bohemia and a more trendy, accessible avant-garde. After all, it is not by accident that cautious Raushenberg wins the coveted Venice Biennial in 1964 rather than a flaming Andy Warhol, too threatening for the "frontiersman" mythology. The fact that, as Tom Crow said beautifully, "the avant-garde serves as a kind of research and development arm of the culture industry: it searches out areas of social practice not yet completely available to efficient manipulation and makes them discrete and visible," is not really addressed in the specificity of this case. What is lacking in the end is a closer connection between the explosion of avant-garde art forms and the political and symbolical needs of a "New Frontier" mentality (which talked about innovation, imagination, challenges) in order to understand that in the end it was the avant-garde—more readily than the Bohemia—which participated in filling the cultural gap along the missile gap dear to the Kennedy administration. Without a political understanding of the connotations embodied in their activities, many of them were giving a sense of false security and contaminated freedom. Artists were often involved in so many self-centered "happenings" that they did not really know what was happening. This otherwise well-documented book does not help us much in tracing the difficult positioning of artists' productions, artists who were often unaware of the involvement of their apoliticism in a developing cultural politics.

Rather than a juxtaposition of social and political facts in an "in context" mode, it would have been nice had Banes discussed how specifically the construction of some art works—happenings, films, dance—were or were not participating in fulfilling the dreams of the administration and American society during the last gasps of the Cold War, by reducing or not reducing the famous cultural gap. The author, for example, could have been more cautious in discussing the subversive quality of the collapse of High and Low in the arts in light of the fact that this particular angle was the cherished line of Arthur Schlesinger, Jr., writing about "New Frontier" strategies for the president. On the surface, subversive attitudes can appear oppositional until one aligns them with the new needs and discourses of the "new liberal" administration. The blending of High and Low in Pop art became their iconic discourse against "intellectual" European modernism. This type of caution and discrimination would have allowed the differentiation of the many rich layers of cultural productions she described, but which, as it stands,

threatens to become the equivalent of the 1950s gobbling blob.

SERGE GUILBAUT

University of British Columbia

DANIEL M. FOX. *Power and Illness: The Failure and Future of American Health Policy*. Berkeley and Los Angeles: University of California Press. 1993. Pp. viii, 183. \$20.00.

In this provocative book Daniel M. Fox brings an informed voice to the debate over health care in the United States. He contends that priorities set a century ago to deal with acute infections still dictate health policy even though chronic diseases have long been the leading cause of death and disability. The result is a policy that is increasingly expensive and inadequate.

A century ago physicians alone set health policy and they had three priorities: more scientific research, more hospitals, and medical schools that emphasized laboratory science and clinical training in teaching hospitals. They were little concerned with helping the poor pay for medical care. Although incremental reforms have been made, patterns of authority remain unchanged. Only in times of crisis has the government turned its attention to health policy. Fox argues that health, so vital to maintaining productivity and civic and family life, is too important to be left to the experts.

The author traces developments in health policy from consensus at the beginning of the twentieth century to disarray at the end of it. Because there was no match between policy and reality after 1920 when chronic diseases became dominant, much of the generous funding for health care from both public and private sources was misspent. Policy makers assumed that tactics proven effective against infectious diseases would work against chronic ones as well, the discovery of insulin as a treatment for diabetes being a case in point. He calls cortisone, discovered in a laboratory in 1949 and tested at the famous Mayo Clinic, "a metaphor for health policy that looked to the past for its priorities . . . [one that] exemplifies the linkage of power and illness" (p. 55). Even after cortisone proved to be no more effective than aspirin in treating rheumatoid arthritis, policy makers who emphasized the treatment of acute phases of chronic diseases rather than their long-term consequences touted it.

By 1950 the mismatch between the allocation of resources and human needs was thoroughly rationalized. A compromise reached during the next quarter century accommodated patients with chronic illness within the existing structure. Insurance covered most hospital costs, but did nothing to help families manage long-term disabilities. Response to the AIDS epidemic illustrates the disarray. It is a chronic disease that affects primarily young people ineligible for

Medicare, the only social insurance program for health care paid for by taxes on everyone who works.

There is no quick fix for health care, but Fox suggests two policy goals: a redistribution of funds from infrequently used acute care services to programs that manage chronic diseases and injuries, and more attention to prevention. He would reduce the number of hospital beds by 40 percent and change the ratio of subspecialists to primary care physicians, now at about three to one, to one to one over a period of several decades.

A well-known medical historian, Fox is president of the Milbank Memorial Fund. His experience in research and public affairs (he has worked in state and federal government and in universities) has convinced him that "Americans should share more responsibility for taking care of each other" (p. 141). The average citizen should read this book.

ELIZABETH W. ETHERIDGE
EMERITA
Longwood College

ROBERT E. BOTSCH. *Organizing the Breathless: Cotton Dust, Southern Politics, and the Brown Lung Association*. Lexington: University Press of Kentucky. 1993. Pp. x, 228. \$29.00.

The Brown Lung Association (BLA) was founded in 1974-75 to address occupational health problems in southern textile mills, seeking to clean up the workplace and obtain compensation for the victims of byssinosis ("brown lung"), a chronic disease resulting from long-term exposure to cotton dust. At its height in the late 1970s the BLA numbered only several thousand members, and it collapsed abruptly in the early 1980s. Nonetheless, in the years since it has attracted considerable interest, of which this book is the latest product.

Robert Botsch came to his subject as a "participant-observer" of a local BLA chapter, and his sympathy for the BLA's organizers, staffers, and members pervades the volume. Nonetheless, his judicious account sees the story of the BLA largely as one of failure. The group helped numerous byssinosis victims obtain benefits theretofore denied them, lobbied for regulatory reform, and raised southern public awareness of a social problem previously invisible. Its larger goal, however, to "empower" marginalized people by creating a continuing "grass-roots" organization working for social transformation, was never achieved. Throughout its existence the BLA was effectively run by its young, college-educated activist staff, and largely funded by private and (fatally) federal grants; the Ronald Reagan administration's withdrawal of federal support effectively produced its collapse. The members, chiefly disabled retired workers, were passive, transient, and dying; the organization was unable to broaden its support in the larger southern working class.

Why did this grass-roots organization fail? Botsch

concludes that the members were victims of a "culture of powerlessness" that made them necessarily dependent on the aid of concerned outsiders. "Powerlessness," though, is surely too extreme a term. Supported by family, community, and church, often enjoying personal access to powerful individuals, able to appeal to a broader public sharing their "moral economy," southern workers were in fact far from "powerless." Yet the organizers of the BLA failed to tap into those indigenous resources; heirs to the community-organizing ethos of the 1960s, they saw traditional institutions and relationships as inadequate, if not corrupt, and preferred to by-pass them by using their command of the modern culture of media and bureaucracy. In this realm, where the staff was so much at home, workers felt adrift; some of Botsch's most excruciating stories concern members' attempts at (to us) simple tasks such as writing letters, tasks designed to encourage individual initiative but that often forced members into slavish reliance on staff members to compose the text.

As traditionalists enmeshed in the machinery of industrial society, members needed the staff, but they also needed the strength of their larger culture to sustain themselves. Convinced that "empowerment" was a gift to be bestowed by a liberal "vanguard" on the "powerless," the organizers and staffers of the BLA never really understood that. Neither, in the end, does Botsch. It is a tribute to the honesty of his book that it suggests conclusions that, ultimately, its author is not prepared to make.

DAVID CARLTON
Vanderbilt University

JIMMY M. SKAGGS. *The Great Guano Rush: Entrepreneurs and American Overseas Expansion*. New York: St. Martin's. 1994. Pp. xii, 334. \$49.95.

This book by Jimmy M. Skaggs assaults the sense of smell and the sense of humor. A study that covers everything anybody might want to know about guano, "the dried excrement of seafowl" (p. 1) and the miracle fertilizer of its time, suggests that the search for guano involved "the scent of scandal" (p. 17) and/or "the lingering odor of scandal" (p. 53). Then there were entrepreneurs who "sniffed about the high seas for guano" (p. 35) and entrepreneurs who "sniffed about seas close to home for bird droppings" (p. 91). Apparently, entrepreneurial tactics did not vary from the Pacific to the Caribbean. If any doubts remain, consider the chapter titled, "Peddling It: The Aroma of Profit."

It is not easy to be serious about a book that also includes chapters titled: "Seward's Outhouse," "Shoveling It," and "Guano Happens." Skaggs, however, makes a serious attempt to demonstrate the "myth of laissez faire" (p. 1) on the part of the American government when it came to finding guano and making it available to farmers along the Atlantic coast who faced declining productivity and prices.

Eastern farmers could not compete with those working the vast tracts of land in the interior.

Guano from Peruvian offshore islands began to arrive at Atlantic ports in the mid-1840s at a price of about \$30 a ton. Demand for limited quantities forced the price to rise in the pre-Civil War era. This trend continued despite Congressional action in 1847 to include guano on the duty-free list. This response from Congress and the subsequent Guano Islands Act of August 1856 (authorizing protection for Americans who might discover deposits of guano) may indeed provide some indication that a laissez-faire approach did not apply to guano. Yet it requires one giant leap of faith to accept the contention by Skaggs that "the ponderous machinery of the United States government was nudged into motion, inexorably toward empire" (p. 17) by the search for guano.

Skaggs covers every last detail of the scramble to find guano in the period from 1856 to 1902. His treatment is made a bit difficult to follow by the contrast between the bibliography and the footnotes. Skaggs provides an impressive thirty-six pages of sources (padded a bit by the separate listing of more than forty items from the *New York Times* and about thirty-five items from the *Baltimore Sun*) but uses what he terms the "scientific" (that is, short) style in forty-three pages of footnotes. Every citation of a book, for instance, includes only the author's last name and page numbers.

This book does make an important contribution. "Peddling It" details the development of the fertilizer industry in the United States in the late nineteenth century; "Shoveling It" describes the horrible working conditions on the guano islands. As Skaggs maintains, "No nineteenth-century job . . . was as difficult, dangerous, or demeaning as shoveling either feces or phosphate" (p. 159). Finally, in "Navassa Island," Skaggs specifically examines the conditions on a two-square mile "corporate 'Devils island'" (p. 169) near Haiti and the riot in September 1889 by the labor force of freed slaves and other African Americans. After an investigation, President Benjamin Harrison commuted to life in prison the death sentences of three men who had been convicted of murder.

ALLAN SPETTER
Wright State University

DAVID F. TRASK. *The AEF and Coalition Warming, 1917-1918*. (Modern War Studies.) Lawrence: University Press of Kansas. 1993. Pp. xi, 235. \$29.95.

Most histories of the American Expeditionary Force (AEF) in World War I reflect the assessment of its commander, General John J. Pershing, who asserted that the operations of the AEF were decisive in the war, assuring victory. Pershing attributed any difficulties he faced to a bumbling War Department at home and perverse interference from Allies abroad. In recent years, historians have produced interpretations less complimentary both to the American forces

and their commander. David F. Trask advances this revisionist view further still, concluding that American inexperience and flawed tactics jeopardized its success in combat operations. Furthermore, Trask argues that Pershing might not have survived a more prolonged war due to his stubborn self-righteousness, unwillingness to correct mistakes, and stormy relationship with Allied military and civilian leaders, which undermined coalition warfare.

To bolster his thesis, Trask not only synthesizes existing scholarship but also utilizes an underused body of primary source material, the seventeen-volume *United States Army in the World War 1917-1919*, which includes many German as well as French and British records. Recognizing that official American military accounts have often ignored negative features, Trask views the AEF from the much-neglected perspective of grand tactics, the inter-Allied application of military power, and assesses the campaign of 1918 by viewing the actions of the coalition armies through the eyes of the highest ranking field commanders.

In keeping with President Woodrow Wilson's war aims, General Pershing's plan was to mobilize an independent army that would fight under its own flag with its own sector of the western front according to its own doctrine, with its own commanders, staffs, and services of supply. It would strike the decisive blow, giving Wilson the leverage needed to dictate the terms of peace. The policy diverged fundamentally from the wishes of the British and French, who wanted "amalgamation," or integration of American units as replacements in Allied divisions. This expedient would introduce American manpower quickly into the war. But Pershing refused any concessions that would interfere with formation of an independent army.

Trask charges that Pershing's decision delayed the AEF's entry into combat and guaranteed that the AEF could not mount a large-scale offensive until 1919. Furthermore, it jeopardized Allied operations, which depended on shock power, a series of massive, coordinated attacks, to impose terrible attrition on German forces and cause their collapse in 1918. Another doctrinal difference arose over Pershing's insistence on the "open warfare" concept. The Allies saw attrition and not maneuver as the logical key to eventual victory. Pershing disagreed; his training emphasized the maneuvering rifleman.

The operational focus of this study is on the two campaigns of St. Mihiel and the Meuse-Argonne. In both, American inexperience, ineffectual commanders, flawed tactics, and poor planning on Pershing's part combined to make these "bitter" victories. Trask argues that the AEF suffered for the rest of the war from Pershing's insistence on conducting an independent American operation against the St. Mihiel salient. For the inadequately trained American divisions, it was too large an undertaking.

During the Meuse-Argonne campaign, Pershing's

offensive stalled after only an eight-mile advance. Again, undertrained troops, inexperienced commanders and staffs, and the failure to coordinate infantry and artillery partially caused the breakdown. In addition, Trask cites Pershing's poor planning. He did not give enough attention to the difficulties associated with a narrow front and difficult terrain. The major value of the AEF was not in its combat performance but rather that it took over large sections of the inactive front, allowing the English and French to relieve their own veteran divisions and create an almost inexhaustible reserve.

PHYLLIS ZIMMERMAN
Ball State University

WILLIAM F. TRIMBLE. *Admiral William A. Moffett: Architect of Naval Aviation*. Washington, D.C.: Smithsonian Institution. 1994. Pp. x, 338. \$29.95.

Only in recent years has the history of naval aviation begun to receive the attention it deserves. This biography, as well as his *Wings for the Navy: A History of the Naval Aircraft Factory, 1917-1956* (1990), establish William F. Trimble as a leading writer in the field. To Trimble, William A. Moffett was "the essential" leader who shaped the development of naval aviation in the 1920s and 1930s. According to Trimble, Moffett waged a three-front war to achieve his ends. Within the navy he restrained pilots who sought a separate naval aviation corps, arguing that aviation had to remain an "auxiliary arm" of the fleet for scouting, direction of gunfire, and attacking enemy ships. At the same time he fought against entrenched bureau chiefs who saw Moffett's new Bureau of Aeronautics as a threat to their prerogatives and a drain on the navy's limited resources. On a third front Moffett battled proponents of an independent air force led by General "Billy" Mitchell.

Successful against all foes, Moffett dominated naval aviation for over a decade. The arms limitation treaties of the Washington Naval Conference and a Congress reluctant to appropriate money for the military after World War I meant stagnation for most of the navy, but Moffett used political connections to obtain equipment and men for aviation.

Trimble has delved deeply into all pertinent records of Moffett's multifaceted work and organizes his study by area of activity, including engine acquisition, racing, airship development, personnel, and Moffett's conflict with Mitchell. The result is a clear and concise biography in which Moffett emerges as stubbornly tenacious in pursuit of his goals, even when he was possibly wrong as when he promoted rigid airships and flying-deck cruisers.

Beyond his determination to advance naval aviation, one learns little about Moffett in this study. The admiral "assiduously segregated his personal life and professional career into two spheres" (p. 97), and despite interviews with both of Moffett's surviving

children, Trimble includes only a few paragraphs about Moffett's family life or avocations.

This limitation aside, Trimble's book is certain to stand as the standard biography of Moffett. With Clark G. Reynolds's *Admiral John H. Towers: The Struggle for Naval Air Supremacy* (1992), it is indispensable to understanding the development of naval aviation in the first half of the twentieth century.

JAMES C. BRADFORD
Texas A&M University

JOSEPH E. HARRIS. *African-American Reactions to War in Ethiopia, 1936-1941*. Baton Rouge: Louisiana State University Press. 1994. Pp. xii, 185. \$35.00.

African Americans' vigorous affirmation in the post-civil rights period of an African identity and global unity produced vast awareness of the power and persistence of race-first philosophies in black life. It also prompted an outpouring of revisionist scholarship on black nationalism by authorities on black thought such as John Bracey, August Meier, and Elliott Rudwick. A subject that sometimes surfaced in these reinterpretations of "separatist sentiments and solutions" but seldom researched was the reaction in the 1930s of U.S. blacks to Italy's plundering of Ethiopia, the world's last outpost of African rule.

Two decades after scholars discovered the depth of black responses to war in Abyssinia, historians acquainted with Ethiopian and African-American studies have now published books on the impact of neo-Roman imperialism on the world's darker races. Joseph E. Harris's study is the most recent work on black reactions to Italian aggression. His volume, a well-written but wispy work, is derived from interviews and investigation of State Department files and the previously unanalyzed papers of scholar William L. Hansberry.

Harris summarizes the experiences of the first Western blacks in Ethiopia who visited the Solomonic empire after its defeat of Italy in 1896. He observes that relations were extended after World War I when Ethiopian missions to the United States solidified American blacks' identification with Ethiopia and inspired visions of Abyssinia as an African Zion among emigrationists such as the Harlem Jew Arnold Ford.

Ethiopianist theology and symbology, the source of African Americans' Abyssinian affinities and absorptions, is recognized but only cursorily reviewed in Harris's discourse on Ethiopia's pan-African associations. The author appears preoccupied with proving that the Ethiopian Research Council, founded on the eve of the war by Hansberry and other Howard University professors, rather than Harlem-based Africanists, was the crucial connection between African Americans and Abyssinians and Ethiopia's gravitation toward pan-Africanism during the early reign of Emperor Haile Selassie.

In four short chapters Harris catalogues the diverse

petitions, parades, physical clashes, and other pro-Ethiopian actions of peoples of African descent. His synopsis subsumes militant action in Africa, the Caribbean, and America, where pro-Ethiopian activism was most pronounced. It recounts the actions in New York of the NAACP, the Universal Negro Improvement Association (UNIA), and community coalitions such as the Provisional Committee on the Defense of Ethiopia and the Pan-African Reconstruction Association that wished to recruit black volunteers to fight in Ethiopia. With more detail and discernment, Harris describes Ethiopian fund raising and its main figures—Willis Huggins, A. L. King, Robert Harris—and the missions to London and Washington of African-American and Ethiopian envoys, most notably the Howard-educated Malaku Bayen.

Harris shows that African Americans, unlike Italian Americans, lacked sufficient political and economic power to affect U.S. policy or aid Ethiopia's cause. As Harris convincingly argues, however, the greater consequence of black protests was the affirmation of a shared identity with Ethiopia that led to effective U.S. black participation in the nation's postwar reconstruction.

This survey, while fluently written, is thin. With footnotes, its core comprises about 100 pages. Moreover, the story it relates has already been copiously told in my book, *Sons of Sheba's Race* (1993). Sadly, Harris's short and blandly titled version says little that is new and therefore contributes sparingly to scholarship on black identity and confraternity.

WILLIAM R. SCOTT
Lehigh University

JOHN RAY SKATES. *The Invasion of Japan: Alternative to the Bomb*. Columbia: University of South Carolina Press. 1994. Pp. xii, 276. \$27.95.

This thorough monograph deals with the U.S. plans for invading Japan at the end of World War II: Operation Olympic, a fourteen-division assault that was scheduled for southern Kyūshū on November 1, 1945, and Operation Coronet, a twenty-four division attack, scheduled for the Tokyo Plain on March 1, 1946. Skates argues that diplomatic historians have ignored the underlying realities of the final months of the Pacific War. According to the author, one of these realities is that most U.S. decision makers and military planners never considered invasion as an alternative to atomic bombing. They assumed instead that only a series of actions, including a tightening blockade, an escalating conventional bombing campaign, an atomic assault, and finally the landings (if still necessary) would force the obdurate Japanese to surrender. He implies, therefore, that the debate by historians over the necessity of the atomic attacks is simply ahistorical. In this argument, his account follows the logic of the best study on U.S. policy toward Japan in 1945, Leon V. Sigal's *Fighting to a Finish* (1988).

Although Skates criticizes diplomatic historians for

considering continued blockade or invasion as possible alternatives to bombing civilians, he himself tries to demonstrate that the United States could have brought the war to an end cheaply by invasion. He implies therefore that the costly campaigns of close blockade and air bombardment were unnecessary. The main contributions of the book lie in his descriptions of the U.S. war plans and his careful evaluations of their likely results. Skates argues that the large, well-equipped Allied invading forces would have rapidly overwhelmed the Japanese defenders in Kyūshū and on the Tokyo Plain. And his research in the military planners' records of 1945–46 convinces him that U.S. casualties would have numbered somewhere between 30,000 and 125,000. These estimates are much lower than the 500,000 or 1,000,000 that Harry Truman, Henry Stimson, and nationalist historians such as Herbert Feis used in justifying the atomic bombings.

The author attempts to show that the planners' lower casualty estimates were correct for two main reasons. First, he argues that Allied airpower would have destroyed most of Japan's kamikaze planes and suicide boats before they could have hit their targets, while Allied airpower would also have prevented reinforcement of the beaches by infantry and armor after the invasion began. Second, the Japanese generals planned to give battle on or near the beaches, rather than concentrating their forces inland in strong-point bunkers, as they had done during the Luzon and Okinawa campaigns. These latter campaigns had resulted in heavy U.S. losses. Skates makes a judicious case for the ease with which the massive invading force would have prevailed over Japanese forces deployed near the invasion sites. His argument, then, supports by implication those by scholars such as Gar Alperovitz, who believe that alternatives to dropping the atomic bomb did exist, but that Truman chose to ignore them in order to scare Joseph Stalin into postwar cooperation.

But justifiable doubts remain. Japanese morale would have been high when defending the homeland, which the Japanese considered sacred. And Skates himself demonstrates that Kyūshū provided excellent mountainous terrain for cave-style defenses near the beaches. He also may overestimate the ability of U.S. airpower to keep reinforcements from the battlefield, given the U.S. Air Force's experiences in Korea and Vietnam. Skates's shakiest assertion is that the Japanese planned no prolonged guerrilla campaign after the fight ended on the beaches. In the summer of 1945, the Japanese Diet agreed to draft all men between the ages of fifteen and sixty and all women between the ages of seventeen and forty to serve in local militias, known euphemistically as People's Volunteer Fighting Corps. Allied forces might well have faced a bloody guerrilla war if the emperor had decided to continue the fight after resistance ended on the beaches.

These caveats aside, Skates has written a useful

book. Many diplomatic historians and most military historians will find much to interest them in this work.

STEPHEN PELZ
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Amherst

W. DAVID CLINTON. *The Two Faces of National Interest*. Baton Rouge: Louisiana State University Press. 1994. Pp. xiii, 278. Cloth \$35.00, paper \$15.95.

The hardest part of any cross-disciplinary study is getting the level of detail right. A historically grounded work of political science, such as this book, risks telling political scientists more than they want to know about history and less than they want about political science, and giving historians too much political science and too little history. In the compartmentalized world of academe, such studies often review poorly, as the specialists pick them apart and dismiss the authors as dilettantes.

Thus, W. David Clinton is to be congratulated all the more for his thought-provoking examination of the theory and historical application of the concept of national interest in American politics. Clinton begins with an analysis of the diverse meanings that have congealed around the phrase: national interest as *raison d'état*, as the vector sum of separate group interests in a pluralist society, as a transcendent set of values toward which society strives or ought to, as cover for anything policy makers find convenient. He then evaluates the criticisms brought against the idea of national interest: that it is irredeemably vague and subjective, that it is undemocratic, that it confers unwarranted legitimacy on whatever group happens to seize control of the policy process, that it stifles dissent, that in an age of planet-wide problems it fosters a dangerous parochialism.

Clinton illustrates his theoretical analysis with four case studies. In selling the Marshall Plan to Congress, explaining American intervention in the Korean War, shifting some of the burden of containment to allies via the Nixon Doctrine, and insisting on respect for human rights during the Carter administration, American leaders were required to enunciate and defend revised interpretations of American national interests. In a neat summary that also encapsulates his thoughts on the "two faces" of national interest—the one looking inward to the values of American society, the other outward toward power relations with foreign countries—Clinton identifies these initiatives as representing, respectively, national interest as generosity, honor, prudence, and justice.

Clinton breaks no new historical ground—which is hardly surprising for a political scientist—but he effectively adduces historical evidence to support his theoretical analysis. Although historians among his readers might have been comforted by indications of the author's familiarity with certain standard historical works—Michael Hogan on the Marshall Plan, for

example, and Bruce Cumings on the Korean War—their omission leads to no glaring missteps.

Clinton refrains from any strong claims for a single proper definition of national interest. Like a good historian, he allows that as times change, definitions must change. His objective is the more modest one of furthering clear-headed discussion of what the national interest comprises. In this he succeeds most admirably.

H. W. BRANDS
Texas A&M University

THOMAS G. PATERSON. *Contesting Castro: The United States and the Triumph of the Cuban Revolution*. New York: Oxford University Press. 1994. Pp. xii, 352. \$27.50.

Few North Americans expect Fidel Castro's Communist regime to survive the end of the Cold War much longer. As Thomas G. Paterson reminds us in this long anticipated and highly instructive examination of Castro's rise to power in 1959, however, those who predict that with his political demise the U.S.-Cuban relationship will return to its prerevolutionary state are likely to be disappointed. Certainly one should hope this is the case. Paterson demonstrates that U.S. policy toward Cuba reflected the pathologies that, by contaminating much of the globe's periphery, poorly served America's national interest.

The turn in U.S.-Cuban relations from friendship to animosity is a familiar story. Paterson's narrative approach, moreover, will probably not satisfy knee-jerk critics of diplomatic history. It would be unfortunate if either consideration detracted from interest in the book. It supersedes popular and scholarly accounts and will remain the standard for years to come. This is because Paterson brings to the study his wide-ranging expertise in the history of U.S. foreign relations, analyzes Cuban dynamics and actors as well as American, and writes in careful and lively prose.

Paterson's research is likewise outstanding. Although he was obstructed by the U.S. government's resistance to declassifying documents, his exploitation of the Freedom of Information Act has borne fruit. Paterson also has uncovered new material in foreign and business archives and private collections, and he interviewed or corresponded with players in Cuba and the United States. Until archives open in Cuba, probably not much more significant evidence will become available.

Paterson addresses three fundamental questions. His answer to how Castro and his rag-taggle "barbudos" managed to drive Fulgencio Batista from Cuba is the most conventional. Batista's authoritarianism, corruption, and avarice alienated his supporters. Castro was never a knight in shining armor, Paterson shows. From the beginning he was irascible and domineering. But he inspired unwavering loyalty, was an unimpeachable nationalist, and repeatedly outma-

neuvered his anti-Batista opponents. To Paterson, the Fidelistas' triumph was not surprising.

He seems not surprised as well by the answer to his more central question (p. 241): "How did the United States let this one get away?" The government associated American interests in Cuba with a *caudillo* who crushed expectations that U.S. cultural and economic domination had helped to create. Poorly advised by the State Department, military, and CIA, a passive President Dwight Eisenhower did not recognize the problem until too late. Futile efforts to find a "third force" replacement for Batista only allowed Castro additional time to grow powerful. Paterson argues that what followed—the Bay of Pigs, Operation Mon-goose, and primarily as a consequence of both, the Cuban Missile Crisis—were manifestations of Washington's frustration.

Paterson equivocates when answering the third question: what motivated U.S. policy? He alternates between stressing the requisites of maintaining hegemonic control and perceived imperatives *sui generis* to the Cold War. This dichotomy is of fundamental scholarly concern. More important, it will be fundamental to the future of U.S.-Cuban relations.

RICHARD H. IMMERMAN
Temple University

RONALD H. SPECTOR. *After Tet: The Bloodiest Year in Vietnam*. Paperback edition. New York: Vintage. 1993. Pp. xvii, 390. \$13.00.

"War is blind," goes a West African proverb. Ronald H. Spector's account of the bloodiest period of the Vietnam War—April through December 1968—at-tests to that painful adage. At the end of nine months of large and costly battles and intensive diplomacy, the war remained as it had been before: mired in stalemate, no closer to resolution, but having exacted a terrible new toll of death and destruction.

Spector, who served as a Marine Corps field historian in northern South Vietnam during most of 1968, has produced a well-written and well-researched narrative of the war in that bloody year. Unlike most Vietnam scholars, who focus on the first three months of 1968, he concentrates on the period after the Tet Offensive and President Lyndon Johnson's March 31 speech (limiting the bombing and announcing his intent not to seek reelection) because he believes the events of the following nine months, "particularly developments in South Vietnam, were far more important in shaping the course of the war for the next five years than anything done in Washington during February and March" (p. xvi).

Most scholars do neglect the rest of 1968. The year's last nine months do deserve closer treatment. But their relative significance remains debatable. The cumulative impact of inconclusive battles and diplomatic deadlock before, during, and after 1968 proved far more decisive in persuading increasing numbers

of Americans that the war could not be won at an acceptable price or in an acceptable time.

Spector makes several stimulating points. He sees World War I as the most appropriate analogy to Vietnam: both wars quickly became stalemated; neither side acknowledged this fact; yet each side underestimated its adversary's determination and endurance. All that is true. But World War I and Vietnam differed enormously in their political (balance of power versus independence) and war-fighting (trench versus guerrilla) dimensions. Afghanistan seems a more apt analogy to Vietnam.

Spector argues persuasively that the failure of most American military commanders to adapt their operational methods to the peculiar conditions of warfare in Vietnam proved a serious error. A counterinsurgency campaign attuned to the war's fundamentally political nature appears wiser, in retrospect, than the search-and-destroy strategy that ravaged the country and alienated the linchpin of the war: the South Vietnamese people.

Spector concludes that America's failure in Vietnam was "a failure of understanding and imagination" about the United States' limited interests in the face of North Vietnam's and the Vietcong's unlimited war of survival (p. 314). But America's greatest failure in Vietnam was a tragic and costly misperception and misjudgment about the underlying political dynamics in Vietnam and the limits of military power to affect those dynamics.

This book broadens readers' conceptions and understanding of the Vietnam War in 1968. Written with the style and force of Spector's earlier work, *Eagle Against the Sun* (1985), it tells many interesting stories and offers many interesting insights. But the question posed by Spector early on still remains: if 1968 was a critical year, why did the war continue until 1973?

BRIAN VANDEMARK

United States Naval Academy

CANADA

ALLAN EVERETT MARBLE. *Surgeons, Smallpox, and the Poor: A History of Medicine and Social Conditions in Nova Scotia, 1749–1799*. Buffalo, N.Y.: McGill-Queen's University Press. 1993. Pp. xvi, 356. \$39.95.

A British possession since 1713, Nova Scotia did not have a permanent settlement until the establishment of Halifax in 1749. Allan Everett Marble writes about the factors shaping health in Nova Scotia from its foundation to the end of the eighteenth century: war, immigration from both Europe and the American colonies, prisoners, epidemic disease (especially smallpox), and the fluctuating strength of the military presence that produced cycles of temporarily high employment followed by unemployment and increased poverty. When not attending those wounded in battle, the military and civilian medical personnel, with their intentions but limited resources, did their

best to cope with disease among the immigrant poor in the various hospitals on land and at sea. Based on an extensive investigation of military manuscripts and court records, Marble's book makes clear the magnitude of the task and the dedication of the researcher.

Works such as this should present opportunities to examine the conjecture that medicine in the new world was different from the old world. This study does seem to sustain a few of the conclusions found in an earlier and intentionally comparative work edited by Ronald L. Numbers, *Medicine in the New World: New Spain, New France, and New England* (1987). Although not cited by Marble, the contributors to that volume demonstrated that medical practice was diverse on both sides of the Atlantic and perhaps not that dissimilar. Missing in the present study is a discussion of the broader contributions made to colonial practice by medical practitioners in provincial England, where more individuals practiced without local interference and beyond the regulatory grasp of London. Marble's focus on the rapidly changing function and composition of London's medical guilds neglects the fact that the provinces always produced most of the immigrants to the colonies, and colonial civilian or military medical personnel in particular. General practitioners and the apothecary surgeon dominated in provincial England.

Given the nature of Nova Scotia's population, it is not surprising to discover that of the 366 military and civilian medical personnel practicing between 1749 and 1799, 92.8 percent were surgeons, with only fourteen holding an M.D. degree. Medical practice in Nova Scotia, like that in the New England colonies, was quite similar to that of provincial England. Because of the relative absence of complaints about medical services, Marble concludes that the public held the profession in high esteem, but that multiple careers were common among practitioners. Unfortunately, there are no data for the composition of the medical profession in Nova Scotia between 1799 and 1828, the year the first regulations were enacted. Without such data, and considering the apparent lack of bickering between practitioners and the high social standing of the profession, there is little reason to accept Marble's contention that regulation was late in coming. Perhaps the question ought to be why regulation was sought at all, and who its advocates and opponents were.

The reader never learns much about the individuals in this group of 366, their interaction as professionals, or their actual medical therapeutics. Unfortunately, the unpaid medical bills reported in the court records used in this study mention only medications and treatments, not diagnoses. Smallpox is the most obvious exception, and Marble suggests that inoculation began in Halifax two decades before 1768, the generally accepted date for the first inoculations in Canada. Even without diagnoses or the actual amounts of therapeutic agents administered, however, some comparisons could be drawn with the

valuable, hard-data-based study by J. Worth Estes, "Therapeutic Practice in Colonial New England" (in Philip Cash, Eric H. Christianson, and J. Worth Estes, eds., *Medicine in Colonial Massachusetts, 1620-1820* [1980], 289-384).

Elements of the chronicle or provincial history are present here, and some readers may find amusement in Marble's feeling the necessity of a footnote to define the "minutemen" at the April 1775 battles of Lexington and Concord (p. 102). This 354-page book includes 163 pages of appendixes, endnotes, a rather limited bibliography, and a useful index. In the last of the nine appendixes, the causes of Nova Scotian deaths between 1749 and 1799 are reported in the following categories: accidental (n=578, with 471 drowning victims), disease (although without an aggregate figure, Marble lists smallpox, with 122 deaths, as primary), infant deaths (n=724), violent deaths (n=196), war (n=107), and miscellaneous (n=26). As with other appendixes, the implications are not thoroughly explored in the text. An average of nearly 200 citation numbers for each of the five chapters makes for tedious checking of the notes. A more economical note system would create space for more thorough analyses of material in the appendixes.

Marble is certainly correct in asserting that this book "represents the first definitive history of medicine and social conditions in Nova Scotia during the eighteenth century" (p. xvi). Granted its limitations, this is a worthy addition to the medical history of an important eighteenth-century North American British colony. Without comparative contexts, however, this study does not appreciably add to our understanding of the differences and similarities of medical practice throughout British America or between the colonies and Britain itself. Nevertheless the volume invites an attempt to integrate the medical history of eighteenth-century British America. Also, studies such as this one lay the groundwork for future comparative studies in the history of health and health-care in Canada and the United States.

ERIC HOWARD CHRISTIANSON
University of Kentucky

SCOTT W. SEE. *Riots in New Brunswick: Orange Nativism and Social Violence in the 1840s*. (The Social History of Canada, number 48.) Buffalo, N.Y.: University of Toronto Press. 1993. Pp. x, 266. Cloth \$50.00, paper \$19.95.

Scott W. See offers a much needed study of the three New Brunswick Orange riots that occurred respectively at Woodstock and Fredericton in 1847, and at the York Point section of Saint John two years later. His research is thorough, and he has managed to present a detailed description of complicated events in a readable narrative that includes a concise account of the social and economic background of the story. This obviously required much patient research and painstaking analysis; it may be doubted that

anyone else will soon attempt to cover the same ground. See's work is therefore likely to remain the definitive study of the subject.

As he tells the story, the small New Brunswick Orange Lodges, originally composed mostly of Irish Protestant immigrants, were inundated by native recruits with the sudden arrival in large numbers of the Catholic "famine Irish" in the 1840s. For the established population of the province, the latter constituted a new and unwelcome experience, while for the New Brunswick Orangemen it bore a striking similarity to conditions they had left behind, wherein the Catholic Irish, in what was for them a reconquest, sought to move back into Ulster by offering higher rents and working for lower wages. The answer of the Irish Protestants to this pressure was the Orange Lodge, an exclusively Protestant fraternal society also attractive to the Loyalist founders of New Brunswick as it stressed devotion to the crown and the British connection.

In one sense, See proves the obvious: that the Orange Lodge and its local branches were popular in character, crossing class lines, but made up principally of workers and farmers. He also establishes the less obvious fact that two-thirds of the New Brunswick Lodge members in the 1840s were natives, not immigrants.

See makes the important point that the rapid growth of the Orange Lodge in the province paralleled the emergence of organized nativism in the United States, manifested in such movements as the Know-Nothing Party. From this the author concludes that Orangeism in New Brunswick was essentially an expression of North American nativism. See carries his case as far as the province is concerned, but he fails to mention that nativism was not characteristic of central Canadian Orangeism. Ogle R. Gowan, the father of Canadian Orangeism, sought, and in the Upper Canada election of 1836 found, an alliance with Irish-born and Scottish-born Catholics. The central Canadian Lodges were always split on the Catholic question, and the great enemy of Catholic schools was not the Orange Grand Master but George Brown, editor of the Liberal Toronto *Globe*.

See's use of the word "nativism" is permissible, as incoming Catholics were strangers, and the natives who joined the New Brunswick Lodge felt threatened. Yet, as his figures demonstrate, about one-third of the membership were Irish-born. Hence the nativism was *de facto*, not *de jure*. The use of the word "racism" is more difficult to justify. Admittedly, discrimination on grounds of religion and culture may have much the same effect as racial discrimination, but they are not the same thing. Catholic converts were welcome in the Lodges, and Orangemen delighted in recruiting former priests. Gowan, the Canadian Grand Master, took pride in his Gaelic ancestors and, on another note, today there exist Orange Lodges in Ghana and Nigeria composed entirely of blacks.

Although it is almost impossible to exaggerate the

suffering of the famine Irish, their Protestant antagonists were ultimately the losers in Boston and New York. This might have happened in New Brunswick had the flood of migration continued at the 1840s rate. The Catholic Irish were formidable.

It would appear that See, in his pursuit of relevance, was remiss in accusing the Orange Lodges of racism: there are dangers in using that word as a synonym for all types of discrimination. Yet such an instance of imprecision does not detract from the otherwise very fine job he has done.

HEREWARD SENIOR
McGill University

KERRY ABEL. *Drum Songs: Glimpses of Dene History*. (McGill-Queen's Studies in Ethnic History, number 15.) Buffalo, N.Y.: McGill-Queen's University Press. 1993. Pp. xx, 339. Cloth \$44.95, paper \$19.95.

This well-researched history of the Athapaskan or Dene Indians in the Mackenzie River basin adds greatly to our understanding of Canada's Northwest Territories. Using archaeological findings, fur trade journals, government and missionary reports, and anthropological studies, Kerry Abel describes in detail important moments in the Dene's past. She reviews the period preceding direct European contact, the impact of the fur trade, the Christian missionaries' influence, and the legal shortcomings of treaties eight and eleven. Throughout her final chapters Abel explains how the Dene (the word simply means "people") have successfully maintained "a sense of cultural distinctiveness in the face of overwhelming economic, political, and cultural pressures from the European newcomers to their homelands" (p. xi).

Archaeologists agree that the Dene's direct ancestors have lived in the Mackenzie River basin for at least 2,000 years. Possibly, Abel suggests, the massive White River volcanic eruptions, which occurred in about A.D. 310 and A.D. 525, caused the Dene to seek new lands. After the explosions, the ancestors of the Navaho and Apache migrated to the south from what is now the Yukon-Alaska borderlands and the other Dene moved further into the Mackenzie basin.

Relying on anthropological studies, Abel sketches out the regional variations among the Mackenzie Dene groups, between the Chipewyan, Dogrib, Slavey, Hare, Gwich'in, Mountain, and Sahtu. The differences between the southern and northern groups have become more pronounced in the nineteenth and twentieth centuries. In the early 1990s, the Gwich'in and Sahtu Dene, in fact, broke their common front with the southern Dene groups to make separate settlements with the Canadian government. The legal deficiencies of treaties eight and eleven necessitated the making of new agreements.

Since the late eighteenth century, the Dene have adapted successfully to each wave of European newcomers, first the fur traders and then missionaries and government officials. Until the introduction of

steamboats in the 1880s, the Dene had the upper hand on the Mackenzie, as they controlled the food supply. The steamboats, and then aircraft in the 1920s, freed the newcomers from a dependence on the Dene for their food. The lack of suitable farmland and limited mining discoveries did, however, prevent a human invasion from the south. Those outsiders who arrived tended to be largely transient, leaving the native population in the majority outside of the large settlements like Yellowknife.

The new diseases inadvertently introduced by the newcomers weakened the Dene. The epidemic of scarlet fever in 1865 affected every band in the north, killing hundreds of people. In the spring of 1928, boats carried an influenza virus from post to post down the Mackenzie. One source estimated that one-fourth of the native population died in a six-week period. Tuberculosis then found its niche among a population badly weakened by the influenza epidemic. Only in the post-World War II era has the health of the Dene improved, and the population is now increasing.

Overall the text reads well, although at times the detail becomes overwhelming. Conscious of this, Abel includes several welcome biographical portraits. Sketches appear of Thanadelthur, a Chipewyan woman who interpreted for the Hudson's Bay Company in the early eighteenth century; of Matonabee, Samuel Hearne's Chipewyan friend who guided him across the Barren Lands; and of the important Métis family, the Beaulieus, who assisted fur traders, then the Roman Catholic missionaries, through their knowledge of the local languages.

This volume succeeds well in conveying the sweep of 2,000 years of Dene history, from the period before European contact to the land claims of the present day. Abel has added greatly to our understanding of the northern First Nations. Despite the pessimistic predictions made by newcomers at the turn of the century, small, dynamic aboriginal societies do continue to exist in Northern Canada.

DONALD B. SMITH
University of Calgary

DIANNE NEWELL. *Tangled Webs of History: Indians and the Law in Canada's Pacific Coast Fisheries*. Buffalo, N.Y.: University of Toronto Press. 1993. Pp. xiii, 306. Cloth \$40.00, paper \$18.95.

Diane Newell's book is the first sustained historical study of native participation in the Canadian Pacific fishery. This previously neglected topic is an important one: coastal and riverine Indians were dependent on fisheries for their sustenance long after white settlement began and the erosion of their access to these resources both for food and cash remains a contested point.

Newell brings to this study a record of scholarship in the history of the Pacific salmon industry and provides an excellent, much needed overview of how

the imposition of fisheries regulations affected the natives who once monopolized and regulated the plentiful marine life of the region.

Aboriginal ownership of the fishery was never recognized by Canada, and an Indian subsistence fishery was "invented" and grudgingly allowed within increasingly strict regulations. Traditional fishing practices were obstacles to the industry profits, while Indian labor was "captured" for its needs. The food fishery was further reduced in the interwar period, although Indian fishers and shoreworkers retained a sizable presence in the industrial fishery and benefited from anti-Japanese measures. Natives lost out, however, in the 1950s, catastrophically after 1968, in the massive regulatory scheme put together to manage the industrial fishery. This process may now have been reversed by recent court decisions that recognize aboriginal fishing rights and give them priority over all but conservation interests.

If all history is contemporary history, this is more contemporary than most. Newell has been involved in Indian legal cases; she found the courtroom "an inadequate, even hostile environment for exploring the tangled webs of history" (p. xi) and so has chosen to publish her research as a contribution to the debate.

This is committed scholarship. British Columbia's Indians are seen as victims of an almost unrelenting state, itself almost invariably controlled by the salmon-processing industry. There is almost no relief from this theme. Any state actions that favor Indians are suspect. The web is not very tangled: it is an unrelieved tragedy. "Although they never surrendered their rights to the fisheries and repeatedly sought protection for their fishing customs and hereditary fishing territories, they eventually became marginalized within one branch of the fishery after another" (p. 4). Now only a tiny portion of commercial licenses remains in the hands of Indians.

Such an interpretation will appeal to many but seems so one-sided as to be suspect, at least in its parts, even within the evidence presented. The "tiny portion" is actually some 19 percent, certainly a decline, but does it define marginalization? The causes of the decline (even greater among plant workers) are complicated and by no means merely the result of the state and its laws. Some of these quite obviously benefited natives and some were even intended to do so. The prohibition of gas motors on the north coast gave an advantage to natives, even if done more in the interests of canners. So did the state's restrictions and later expulsion of Japanese Canadians, actions urged and welcomed by Indians.

Structural changes, perhaps more than governmental action, decreased the competitive position of natives. Market developments, changing technology, demographic alterations, and other forces are mentioned, but they always seem subordinated to a case against the Canadian state. Victims and villains are here, but there is not much complexity, nuance, or

ambiguity of judgment. In taking her brief to the public, Newell has not really left the courtroom.

DOUGLAS COLE
Simon Fraser University

LATIN AMERICA

FRANK FONDA TAYLOR. *To Hell with Paradise: A History of the Jamaican Tourist Industry*. (Pitt Latin American Series.) Pittsburgh, Pa.: University of Pittsburgh Press. 1993. Pp. ix, 239. \$39.95.

Although tourism has become the backbone of Jamaica's economy in the last century, it continues to have social and cultural costs that cannot be measured by tourist arrivals and expenditures. Soaring land prices, prostitution and drug trafficking, and underpaid, insecure menial jobs may be intrinsic to the industry. Moreover, the pattern of foreign control and dependency and the emphasis on black Jamaicans catering to the whims of white foreigners smacks of the old plantation economy and the social relations of slavery. Frank Fonda Taylor's study addresses these key and sensitive issues.

The "infancy" of the tourist industry, between the 1890s and 1914, was marked by efforts to redefine the island's image from a white man's graveyard to a health resort. This change depended on the control of malaria and yellow fever and the promotion of the island by the Jamaica International Exhibition of 1891. The value of tourism was initially conceptualized "in its capacity to indirectly encourage foreign investment" (p. 58) by attracting rich visitors, some of whom, it was hoped, would settle and invest in Jamaica. The tourist trade became dominated by the Boston-based United Fruit Company, formed in 1899 on the basis of Lorenzo Baker's banana business. American tourists came on Baker's boats and stayed in his hotels. By 1912-13, there were 11,318 tourist arrivals, most of them American. Jamaica remained a British colony but became increasingly dependent economically on the United States.

The United Fruit Company virtually monopolized the Jamaican banana and tourist trades. Its new hotel, built in 1904 to accommodate 500 guests, epitomized the expanding enterprise. Owned, patronized, run, and staffed by Americans, "it was totally symbolic of the new economic imperialism" (p. 87). Taylor draws attention to the social and cultural implications of the fact that American investors, staff, and tourists brought a harsh pattern of race relations into Jamaica. The traditions of planter hospitality, which depended on black servility, were reinforced and "the hotels became an arena of subtle black-white confrontation in which the specter of past struggles came to life in a new guise" (p. 89).

Taylor discusses this "foundation period" in detail, but he traces the rest of the history, from 1914 to the 1990s, in only 50 pages. Tourist traffic and facilities for visitors expanded in the 1920s and 1930s, and

65,269 visitors stopped in the island in 1937. Montego Bay emerged as a tourist resort in this period, when many of the finest beaches were alienated from Jamaicans and the island's morality was affected by the pressure to cater to tourists. Many tourists were "arrogant, ostentatious, and coarse," and tourism "elicited an outbreak of touting, begging, and multifarious forms of parasitism" (pp. 152-53).

After World War II the tourist industry was marked by the rapid expansion of air transport. By the mid-1950s about three-quarters of all tourists arrived by air and mass travel changed the clientele from a rich elite to more budget-conscious travelers. By 1965, when the tourist trade brought over 300,000 people and almost \$65 million to Jamaica, tourism passed the sugar industry as the leading foreign exchange earner. Along with growth came increasing concentration of ownership as U.S. airlines bought and expanded hotel chains. Although the Jamaican government promotes tourism and hordes of tourists visit the island—over a million a year by 1987—many Jamaicans feel excluded from the financial benefits and look askance at the social and cultural consequences.

O. NIGEL BOLLAND
Colgate University

CHRISTOPHER WARD. *Imperial Panama: Commerce and Conflict in Isthmian America, 1550-1800*. Albuquerque: University of New Mexico Press. 1993. Pp. xii, 272. \$45.00.

For two centuries Panama was the pivot on which Spain rested its economic ties with Peru and other Pacific Coast colonies. What was good for Spain's imperial needs, however, was poison for Panama. Without the advantage of its geographic location, Panama would have been another backwater section of the empire, remaining largely ignored. Having become since the 1510s the link between Madrid and Lima, the Isthmus of Panama's artificial economic life served the needs of Portobelo and Panama City as fair sites for brief weeks each year when the fleets came. This life disappeared when trade conditions changed and Panama ceased to be an essential imperial cog. In 1751, it even lost the *audiencia*.

The story is fascinating because the Isthmus's life was closely bound to the economy of Spain and thus was very significant during two-thirds of the colonial period. Christopher Ward presents a comprehensive view of the latter's economy within the confines of European development and the intricacies of trade, including the constant threat by corsairs, pirates, and contrabandists, and attacks from national navies in the 1600s and 1700s (chaps. 1-3). The author also covers extremely well the fleet system and Spain's necessity to protect the Isthmus, even in the 1700s when its security was more politically dictated than economically (chaps. 4-6). The "conclusions" and the bibliographic essay show the care that Ward has

put into his study. The author has reconstructed, as well as extant records allow, the dynamics of Panama's life and the transfer of Panama City from its original site to the one chosen to rebuild it in a militarily defensible place.

Contrary to the legendary Isthmian wealth, life in Panama during the imperial years was not easy. Undoubtedly, the area benefited periodically from the arrival of merchants and buyers who flocked to the fairs in Portobelo and Panama City. These visits, however, were not constant. Eventually the vagaries of trade, controlled by European diplomacy and war, discouraged local producers; inevitably, Costa Rica became the supplier of food to Panama. Worse still, when the fleets stopped and the fairs vanished, the developed land in the vicinity of Panama City became an economic albatross in the region's quest to find economic viability.

Based on research in the Archives of the Indies and an array of printed information—some of which, such as the work by Pierre Chaunu and Huguette Chaunu (*Seville et l'Atlantique 1504–1650* [1955–59]), are treasure troves of archival data—this methodically written study is a valuable addition to the literature on the Isthmus. Tables, maps, and plans enhance the presentation of the material. Because this period of Panama's history lacks local archival data, for neither the climate nor the transient nature of Panama's economy were conducive to the preservation of records, these commendations are better understood. That Ward has fully taken advantage of every thread that his exhaustive research produced is laudable.

The only discordant note is in the proofreading. There are typographical errors and misnamed plates that should have been detected. In spite of this flaw, Ward's book is an excellent contribution to the study of Panama.

J. IGNACIO MÉNDEZ
Northeastern Illinois University

JIM HANDY. *Revolution in the Countryside: Rural Conflict and Agrarian Reform in Guatemala, 1944–1954*. Chapel Hill: University of North Carolina Press. 1994. Pp. xii, 272. Cloth \$45.00, paper \$15.95.

The aborted Guatemalan Revolution of 1944 to 1954 has been traditionally seen as a principal casualty of the convergence of economic imperialism and the Cold War, with the focus of interest on the covert intervention by the United States in June 1954 to protect the interests of the United Fruit Company and to halt the spread of international communism. Jim Handy is one of a number of scholars who are turning away from the international aspects of the upheaval to examine the policies, the dynamics, and the internal impact of the revolution itself. His book is a thoroughgoing, archivally based treatment of agrarian reform, a program begun haltingly under the administration of Juan José Arévalo and pursued intensely under Jacobo Arbenz Guzmán. Although

Handy focuses on the implementation and the impact of the Agrarian Reform Law of 1952, he successfully places the "revolution in the countryside" in the context of national politics, in the process revising our understanding of the collapse of the Arbenz regime.

His basic argument is that the city-based Guatemalan reformers were generally well intentioned and sincere but did not understand the complexities of the countryside. In short, they attempted an urban revolution in a rural country (p. 48). Moreover, the implementation of agrarian reform was inhibited by contention between the multiple national political groupings and trade-union and peasant organizations. Handy demonstrates effectively, with numerous and varied local examples, that the agrarian program opened a kind of Pandora's box. The land petitioners' claims and counterclaims elicited conflicts between regions, between agricultural (*finca*) workers and communities, between ethnic groups (for example, Indians and non-Indian *ladinos*), between Indian communities and municipalities, and within Indian communities themselves. He tells us that agrarian officials were often befuddled by the complexities of rural Guatemala; the same can be said for the non-Guatemalanist reader. Handy attributes the failure of the revolution more to internal than to external causes. In his determination to push through the agrarian program, come what may, Arbenz failed to control extremists, including important communist advisors, thus alienating leaders of the new army, who succumbed to the fear of communism and of a militant Mayan peasantry. Internal conflict thus made the regime an easy prey for the CIA-backed "liberation" under Colonel Carlos Castillo Armas.

The book would have benefited from more engagement with other historians and theorists. For example, Handy mentions three other interpretations (p. 139) of the rural impact of the revolution, but only in passing. He introduces at the outset a debate over whether or not there existed a peasant mode of production, without returning again to the theoretical issues. He touches on but does not develop intriguing questions of ideological continuity. It is clear that the liberal/positivist tradition weighed heavily on the dominant individualist and assimilationist ideology of the revolutionaries, yet the provenance of an important countercurrent that led to special recognition of indigenous communities in the Constitution of 1945 is less clear. These constitutional provisions guided pro-indigenous reformers, yet Handy also identifies them as conservative. Such matters of context and continuity can be regarded as minor defects in what is an important and engaging study of an immensely complicated subject.

CHARLES A. HALE
University of Iowa

SOL SERRANO. *Universidad y nación: Chile en el siglo XIX*. (Imagen de Chile.) Santiago de Chile: Editorial Universitaria. 1994. Pp. 276.

Sol Serrano has written the definitive history of the University of Chile in the nineteenth century, that is, the period of foundation, consolidation, and transition from a non-teaching academy to a teaching institution. The University of Chile has been the subject of numerous studies, including most recently Rolando Mellafe's *Historia de la Universidad de Chile* (1992), and Bernardino Bravo Lira's *La universidad en la historia de Chile* (1992), but Serrano's work far outpaces these general studies with a detailed, thoroughly researched, and theoretically sophisticated book that sets new standards for histories of universities in nineteenth-century Latin America.

The University of Chile was no ordinary institution. It was the institution of higher education in Chile until the creation of the Catholic University in 1888. From 1843 (the year of its inauguration) to 1879 (the year it was recognized as a teaching institution), the University of Chile monitored the entire educational system, cultivated and concentrated research, and established the study of law, medicine, and engineering with an emphasis on serving national needs. Most importantly, it served as a recruitment ground for the country's expanding public sector. Although it included a faculty of theology, the institution was decidedly secular, and public. The increasingly powerful liberal state viewed the university as the key to modernizing and democratizing Chilean society. True to its intentions, the government provided consistent funding and political support, defending the university from congressional and church attacks until the institution was standing solidly on its feet and became, as historian Guillermo Feliú Cruz once put it, "la universidad de América" on account of the numerous Latin Americans who studied or taught there.

Serrano argues that the transition from a non-teaching academy to a teaching institution was due to internal developments as university officials tried to fulfill their government mandate for the creation of a national educational system. It also was a response to the larger processes of modernization and nation-

building. University and government leaders (often one and the same) determined that Chile needed to train and place its own professionals in order to reverse the more expensive practice of importing foreign talent. They did so even in the face of a sluggish market demand for Chilean professionals, and despite the elite's traditional reluctance to join professions that lacked prestige (except for law). Medicine and engineering, in particular, underwent a difficult but ultimately successful process of establishment and professionalization. Thus, Serrano concludes that the university provided a strong example of "reform from above" not unlike that of the late-colonial period, and she gives a positive assessment of the state's determination to pursue long-term goals despite short-term political costs.

In addition to a great deal of detail about budgets, personnel, and degrees, and a plethora of statistical information, this book places the university in the larger context of nineteenth-century Chilean history (with some comparative references). It shows that the university was at the center of educational and political debates because it was rightly perceived as a powerful instrument for the transformation of society. Perhaps the most contentious debates involved church and state, but Serrano's examination of congressional records, press articles, and other documents reveal significant tensions between the legislative and executive branches of government. The focus on the university shows just how strongly centralized and presidentialist the Chilean political system really was.

This book is meticulously researched and lucidly written. The author has mined one previously underutilized archival source: the Archivo del Tribunal del Protomedicato, which contains a wealth of materials for documenting not only the professionalization of medicine but also the complex relationship between state and society during a period of bureaucratic centralization. Although the bibliography could have been broken up into more useful categories, the documentation is superb. This book represents a welcome addition to the literature on nineteenth-century Latin American history.

IVÁN JAKSIĆ
University of Notre Dame

Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed.

GENERAL

WILLIAMSON MURRAY *et al.*, editors. *The Making of Strategy: Rulers, States, and War*. New York: Cambridge University Press. 1994. Pp. xiv, 680. \$34.95.

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- WHITNEY, ELLEN M., compiler. *Illinois History: An Annotated Bibliography*. Edited by JANICE A. PETTERCHAK and SANDRA M. STARK. (Bibliographies of the States of the United States, number 4.) Westport, Conn.: Greenwood. 1995. Pp. xxxiv, 603. \$85.00.
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Other Books Received

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- BANUAZIZI, ALI, and MYRON WEINER, editors. *The New Geopolitics of Central Asia and Its Borderlands*. Bloomington: Indiana University Press. 1994. Pp. 284. Cloth \$39.95, paper \$15.95.
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ARTICLES

TO THE EDITOR:

I thought Thomas C. Holt's article, "Marking: Race, Race-making, and the Writing of History," was a masterful attempt to address a serious moral and social problem confronting the United States and the world [AHR, 100 (February 1995): 1-20]. His selection of quotes from both W. E. B. Du Bois and Franz Fanon demonstrated the epiphenomenal strength of racism as it transcends centuries, territorial boundaries, and human generations. Du Bois, a man of the nineteenth and twentieth centuries, and Fanon, a man of the twentieth, both descendent Africans but one an American and the other a Martiniquen, were both racially "marked" by a world phenomenon peculiar to the white world, white supremacy.

It is in the Western world, and particularly in the United States, that racism (white supremacy) reveals itself in a most sinister and diabolical form. Whether it is evident in what the author terms "everydayness" or as *aberrant behavior* in response "to choices made in a context of global economic and political forces" (p. 6), the ideology of white supremacy (racism) has phenotypically "marked" African descendents and set them apart from descendent Europeans within the "cultural communion of whiteness." Holt's resort to the works of Du Bois and Fanon and his citing of the Sam Hose lynching and the East St. Louis holocaust against African descendents speak to the peculiarity of racism as acted out in the United States as in some bizarre psychodrama.

It is on this point that Holt's masterful attempt falls short. Rather than elaborate on, argue for or against the "psychological paradigm" of Du Bois in which he

states that "'in the fight against race prejudice, we were not facing simply the rational, conscious determination of white folk to oppress us; we were facing age-long complexes sunk now largely to unconscious habit and irrational urge'" (p. 5), and, in note 11, criticize Joe Kovel's *White Racism: A Psychohistory* as too "metaphorical," Holt, in the balance of the article, elects to be more apologetic for racism than empirical in a search for its remedy.

For example, to argue that "white Americans embraced minstrelsy, therefore, not simply to revile black people; that motive alone could hardly have sustained their century-long love affair with the form" is to argue apologetically that blackface minstrelsy, in all its sinister mocking and viciousness, was not endemic to the ideology of white supremacy but was part and parcel of its socialization process for both native and immigrant whites. Yes, as Holt states in reference to the socialization process, "minstrelsy soothed white anxieties, however, at the cost of reinforcing black stereotypes and institutionalizing racist ideas and images for generations to come" (p. 16). But behind his apology hide the genocidal acts against African humanity as acted out by those who were impacted by that "century-long love affair."

Holt as an apologist is evident as well in a note where he indicates that "the audiences for minstrel shows were overwhelmingly urban, working-class northern white men—often immigrants or recent native migrants to the city" (p. 16, n. 41). Does he mean that because they were northerners, the lynchings, pogroms, and burnings—things peculiar to certain regions of the country—were not an expression of their hate for African descendents? His readers have simply to read Leon Litwack's book, *North of Slavery*, or my recent book, *Destructive Impulses* (1995), to see that "northern white men" and women were not immune to such diabolical acts of inhumanity.

As I wrote in a 1992 piece ("Imagery and Race—The Politics of Fear in America"), minstrelsy's message for whites during the antebellum period was one of hate and fear developed to perfection in urban and rural theaters and capitalized on by extreme elements in society. It was used to influence public policy and to convince fearful whites of the socioeconomic dan-

gers of the movement to abolish slavery, especially the genetic threat posed to the white race in terms of miscegenation. As Jean H. Baker stated so well in Chapter 6 ("The Negro Issue: Popular Culture, Racial Attitudes, and Democratic Policy") of her book: "Bound together by their color, audiences experienced the collective exaltation of their whiteness (and maleness). The key to minstrelsy rested on the expectations whites held of blacks and the domination this impersonation conveyed" (*Affairs of Party: The Political Culture of Northern Democrats in the Mid-Nineteenth Century*, 1983, p. 220). It was this white "bonding" (as a result of fear) that permitted black-face minstrelsy to survive both the internecine conflagration of the Civil War and the collapse of Reconstruction. Not that of a "love affair."

As a man of the South and as a historian, Holt is, I am sure, familiar with Joel Williamson's book *The Crucible of Race*, and Holt's encounter with a southern, "politically correct world" (pp. 19–20) in the 1950s was the living manifestation of Williamson's "cultural communion of whiteness" as described in that book. What the book points out, and it is implicit in Holt's "Marking," is the relentlessness of the "communion" in phenotypically "marking" African descendents and defining them as "outlandish" and beyond the pale. The same power, capital, racial privilege, aggressiveness, and other methods of control that the author cites as in existence in the 1950s and earlier are evident today. The only difference is that racism is more sophisticated and subtle but, nevertheless, relentless.

Holt's masterful attempt could have been exactly that if he had taken his "cultural paradigm" and argued it in the vein of Williamson's "cultural communion of whiteness," rather than assuming an apologetic stance on such a serious issue that impacts the life chances of African descendents in the United States. Although in paragraph three of page 20, Holt alludes to white supremacy's (racism) position over a century ago and states that "there is much at stake," he apparently failed to see that what he articulated for the 1950s is happening today. The assault by the American Right on the fruits of the civil rights movement, under the guise of its "Contract with America," is the ongoing perniciousness of "age-long complexes sunk now largely to unconscious habit and irrational urge" as fostered by the "cultural communion of whiteness."

Thomas C. Holt did write rather eloquently in reference to self-emancipation as initially sought by both Du Bois and Fanon before they could go on to articulate emancipation for others. He wrote: "they realized that despite the forces arrayed against us, we—especially historians—must provide some of the materials for that self-fashioning and thus self-emancipation" (p. 20). Unfortunately for Holt the historian, his material, which started out masterfully,

ended up, as he stated, "in exposing obstacles [rather] than certain paths of liberation" (p. 18).

A. J. WILLIAMS-MYERS
The College at New Paltz,
SUNY

THOMAS C. HOLT REPLIES:

I can only express surprise and dismay that A. J. Williams-Myers finds in my essay any "apology" for racism. As best as I can make out, the substance of that so-called apology consists of my dissatisfaction with forms of explanation that find or imply that racism is some innate characteristic of white people—particularly, white Americans. In my view, to argue simply that white Americans hated and feared African Americans or made them the foil for a "cultural communion of whiteness" is at best a description, not an explanation. Indeed, in some formulations—notably Williams-Myers'—such explanation amounts to little more than a tautology: white racists were racists because they didn't like black people. The point of my essay is to suggest not only that the problem is theoretically complicated but how we might begin to address those complications—in both our scholarship and our practice. What's apologetic about that?

THOMAS C. HOLT
University of Chicago

REVIEWS OF BOOKS

TO THE EDITOR:

The correspondence between Piotr S. Wandycz and Antony Polonsky, on the one hand, and Ben-Cion Pinchuk, on the other, occasioned by Pinchuk's review of my book, *Facing a Holocaust: The Polish Government-in-Exile and the Jews, 1943–1945* [*AHR*, 99 (October 1994): 1473–74], compels me to clarify one point of fact. Pinchuk's assertion that "the activity of the Polish government in the face of the Holocaust was 'glaringly meagre' is Engel's conclusion" is false. The phrase "glaringly meagre" is Pinchuk's alone: readers will not find these words anywhere in the book in question. Nor will they find any attempt to reduce the Polish government's activity with regard to matters of concern to Jews to a single facile, denotatively imprecise, value-laden phrase. The subject defies any such simple characterization, and ignoring its nuances and complexities does egregious violence to the historical record.

I shall not comment here about any of the other issues raised in the correspondence, as they too are exceedingly complex and require extended elucidation.

DAVID ENGEL
New York University

Ben-Cion Pinchuk was given an opportunity to reply but has not responded.

THE EDITORS

TO THE EDITOR:

I have just had the interesting experience of reading Robert A. Trennert's review of my book *Apache Reservation* [AHR, 100 (February 1995): 245]. Generally, I would consider it bad form to take issue with a critical review. I must raise a few questions about this one, however, since some of his substantive points require correction.

Some of the problems are trivial, if puzzling. Trennert takes me to task for stating that "Ezra Hayt was Indian Commissioner in 1929 when he actually served in 1877-80 (p. 5)." Oddly enough, I was unable to find any reference at all to Ezra Hayt, or to recall having made one, either on page 5 or elsewhere throughout the book.

He also feels that my allusion to General James Carleton's "no prisoners" policy is incorrect. I am not sure how else to interpret Carleton's order that "all Indian men of that [Mescalero] tribe are to be killed whenever and wherever you can find them" (quoted in Edwin Sabin's *Kit Carson Days*, vol. 2, p. 702).

More troublesome, though, is Trennert's assertion that "Perry implies that the army actually sought to prolong the fighting in Arizona to spur the economy (p. 116)." My main argument is quite the opposite. A good deal of that chapter and others discuss the ways in which the army sought many times to conclude the fighting, while Arizona civilians, such as those associated with the infamous "Tucson Ring," constantly provoked conflicts—much to the army's frustration. The interesting point—or so I thought—was that these roles contradicted certain conventional assumptions.

Trennert seems annoyed at my suggestions that the local citizenry of Arizona acted unjustly toward the Apache. It is perfectly legitimate for him to disagree with my interpretation, but I believe that evidence, ranging from inflammatory newspaper editorials of the time to the many vigilante attacks on peaceful Apache communities, easily substantiates my assertions. On the other hand, I took pains early in the book to disavow any simplistic "good guy/bad guy" dichotomy, emphasizing instead the conditions in which policies and actions arose.

Trennert takes issue with what he considers a Marxist interpretation of events and my failure to cite some decidedly non-Marxist secondary sources. While these may or may not be valid criticisms, I fear that they distracted him to the extent that he not only misunderstood one of the central arguments but stood it on its head. I can only urge that anyone interested in these issues consult the book.

RICHARD J. PERRY
St. Lawrence University

ROBERT A. TRENNERT REPLIES:

In response to Richard Perry's reaction to my review of his book *Apache Reservation*, I stand by my review and admit that I disagree with some points of interpretation. But I am also concerned by some of his assertions. He apparently considers using correct facts to be "trivial." I certainly don't agree. Regarding Commissioner Ezra Hayt, the error appears on page 145. What is disturbing to me is that this factual error along with others conveys the impression that Perry is basically unfamiliar with many historical events. (Most scholars familiar with federal Indian policy would know that Hayt was not commissioner in 1929. Any historian of the Southwest would know that Pancho Villa never raided as far north of the border as Globe.) It would have been easy for him to check for historical accuracy.

Much the same goes for his comments on General Carleton's "no prisoners" policy. This is taken out of context. In his book, Perry (p. 5) implies that Carleton's statement involved men, women, and children. In fact, Carleton made several statements along this line in dealing with both the Mescalero and the Navajo, but always as a tool to force surrender, and it applied only to "hostile" males. Indeed, if one looks at the rest of Carleton's order to Kit Carson regarding the Mescaleros, the final line reads that "you are there to kill them [males] whenever you can find them; . . . [but] if they beg for peace, their chiefs and twenty of their principal men must come to Santa Fe to have a talk there." I agree that this was a brutal policy, but to my mind Perry implies a lot more (for instance, a policy of genocide) than can be supported by the facts. This is why I raised concerns.

Like Perry, I urge anyone interested to consult his book. I also suggest they read other materials.

ROBERT A. TRENNERT
Arizona State University

TO THE EDITOR:

I am afraid that Duane Champagne, in a review of William G. McLoughlin's *After the Trail of Tears: The Cherokees' Struggle for Sovereignty, 1839-1880* [AHR, 100 (April 1995): 581], errs by saying that the late Professor McLoughlin was "the author of several highly regarded books about the Cherokee and had little interest in other historical topics or peoples." The fact is that McLoughlin, who did write a number of books on the Cherokees, was also the author of many other scholarly works, including an epic history of the Baptists in colonial New England, a documentary edition of the writings of Isaac Backus, biographies of Backus, Billy Sunday, and Billy Graham, a provocative book on religious awakenings in American culture, and a short history of Rhode Island, to name only a few.

McLoughlin, who taught at Brown University for nearly four decades before his death in 1992, was

probably best known as a historian of American religion. But he also knew about southeastern Indians (apart from the Cherokees), northeastern Indians (including the Narragansetts and the Wampanoags), and American social and intellectual history (his most popular undergraduate course). His firm command of a wide array of historical and ethnological subjects was brilliantly reflected in his extensive scholarship on the Cherokees and their relations with other peoples—red, black, and white. Bill McLoughlin

knew a lot about the Cherokee people, but he also knew a great deal more besides.

GLENN W. LAFANTASIE
Office of the Historian
Bureau of Public Affairs
U.S. Department of State

Duane Champagne does not wish to reply.

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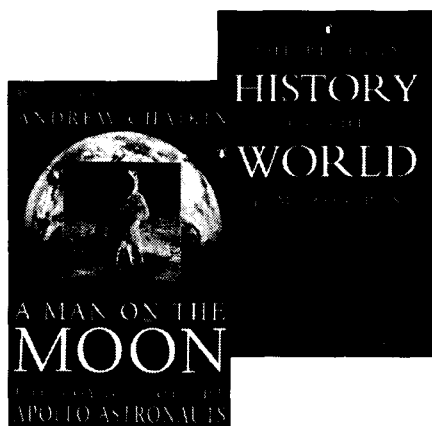
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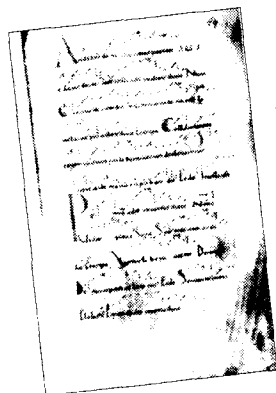
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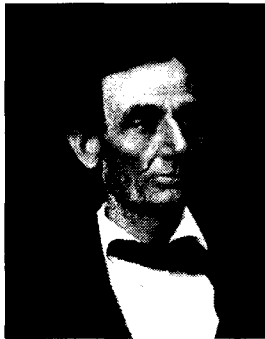
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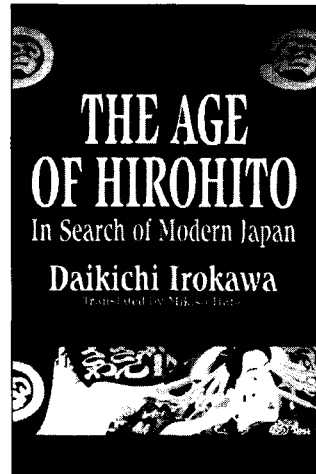
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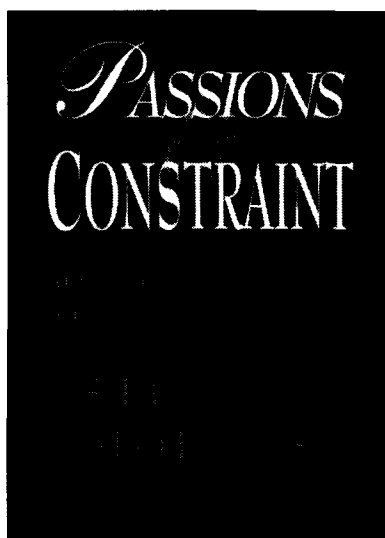
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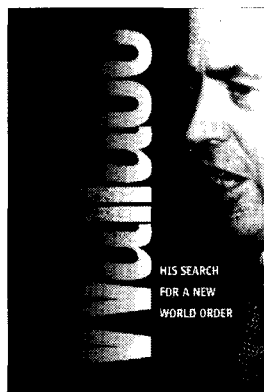
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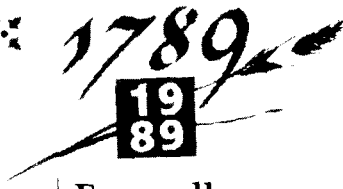
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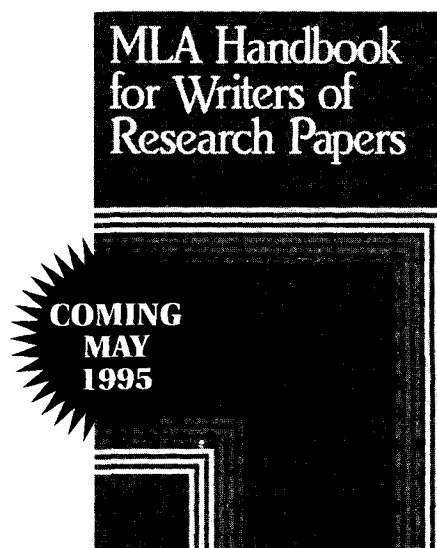
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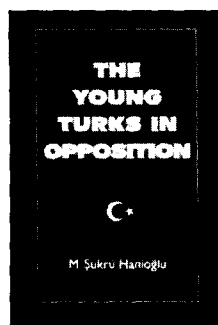
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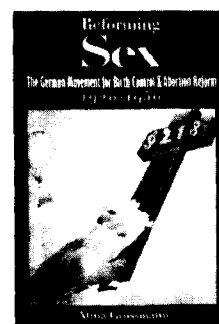
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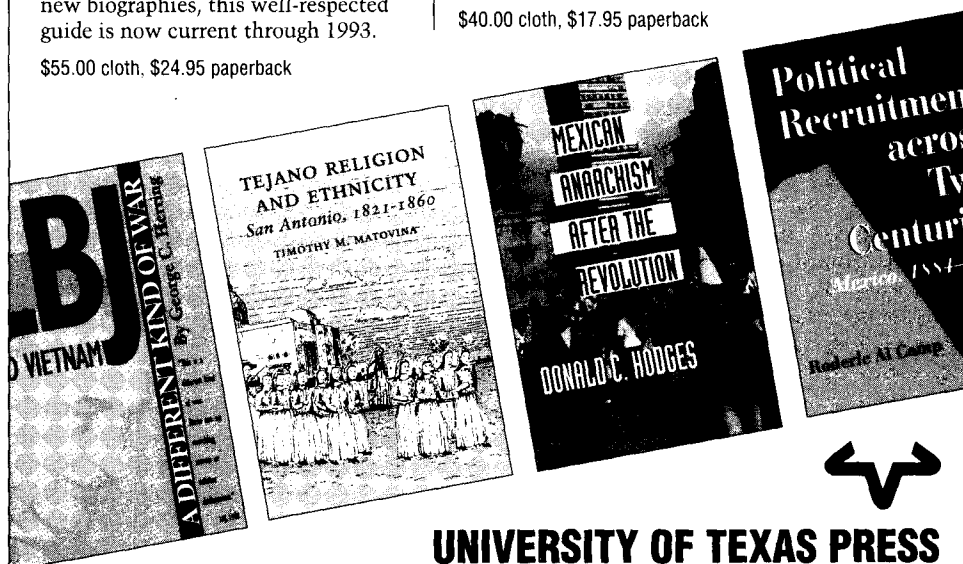
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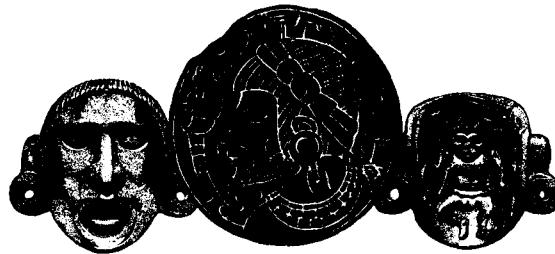
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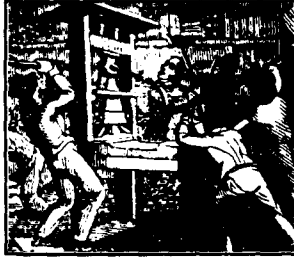
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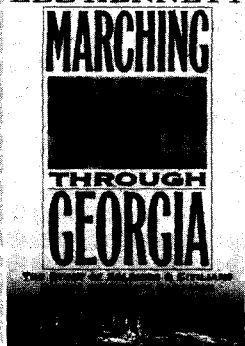
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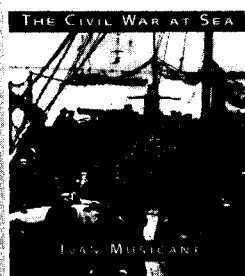
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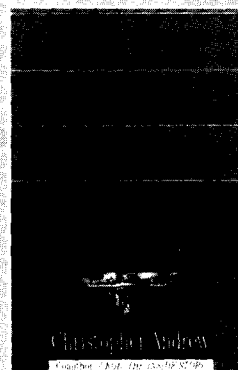
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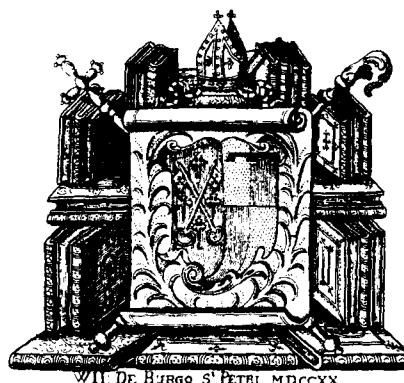
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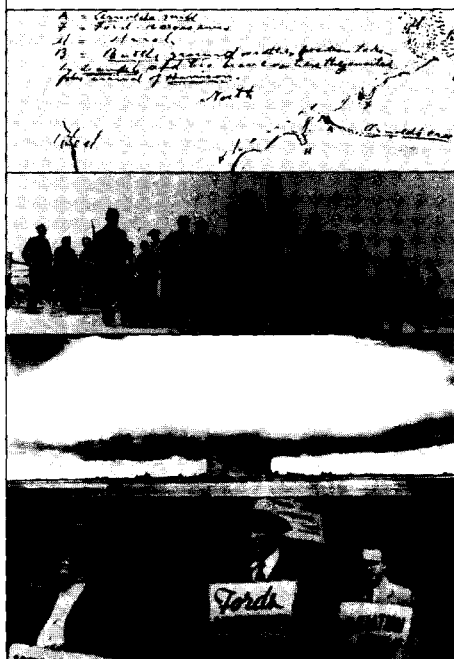
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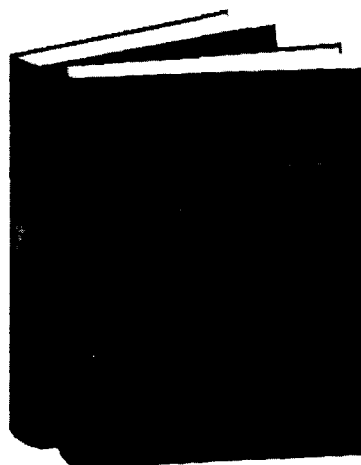
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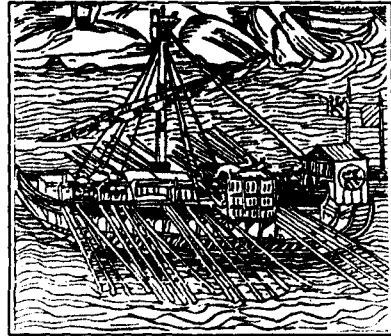
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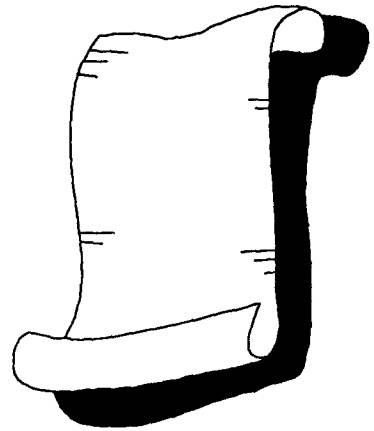
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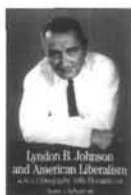
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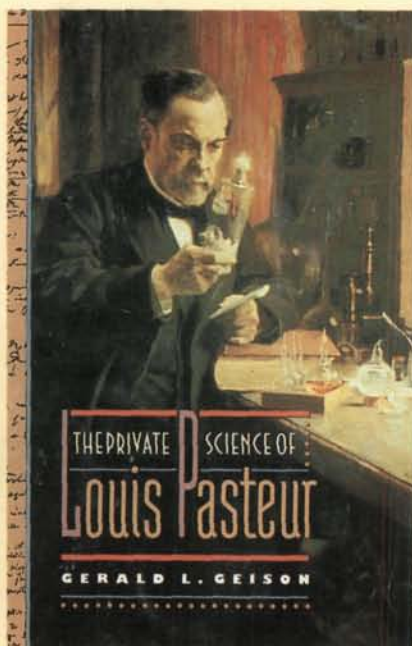
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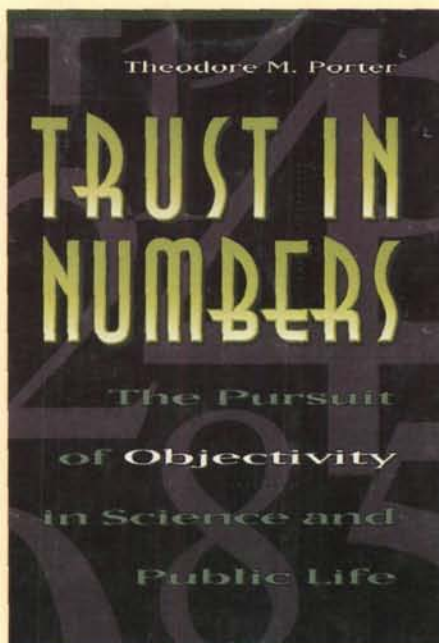
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